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PHILOSOPHICAL TRANSACTIONS

XIV. *New Experiments upon Heat. By Colonel Sir* Benjamin Thompſon, *Knt. F. R. S. In a Letter to Sir* Joſeph Banks, *Bart. P. R. S.*

Read March 9, 1786.

DEAR SIR,

I HAVE at length begun the courſe of experiments upon heat which I have ſo long had in contemplation; and I have already made a diſcovery, which, if not new to you, is perfectly ſo to me, and which I think may lead to a further knowledge reſpecting the nature of heat.

Examining the conducting power of air, and of various other fluid and ſolid bodies, with regard to heat, I was led to examine the conducting power of the *Torricellian vacuum*. From the ſtriking analogy between the electric fluid and heat reſpecting their conductors and non-conductors (having found that bodies, in general, which are conductors of the electric fluid, are likewiſe good conductors of heat, and, on the contrary, that electric bodies, or ſuch as are bad conductors of the electric fluid, are likewiſe bad conductors of heat), I was led to imagine that the Torricellian vacuum, which is known to afford ſo ready a paſſage to the electric fluid, would alſo have afforded a ready paſſage to heat.

The common experiments of heating and cooling bodies under the receiver of an air-pump I concluded inadequate to determining this queſtion; not only on account of the impoſſibility of making a perfect void of air by means of the pump; but alſo on account of the moiſt vapour, which exhaling from the wet leather and the oil uſed in the machine, expands under the receiver, and fills it with a watery fluid, which, though extremely rare, is yet capable of conducting a great deal of heat: I had recourſe therefore to other contrivances.

I took a thermometer, unfilled, the diameter of whoſe bulb (which was globular) was juſt half an inch, Paris meaſure, and fixed it in the center of a hollow glaſs ball of the diameter of 1½ Paris inch, in ſuch a manner, that the ſhort neck or opening of the ball being ſoldered faſt to the tube of the thermometer 7½ lines above its bulb, the bulb of the thermometer remained fixed in the center of the ball, and conſequently was cut off from all communication with the external air. In the bottom of the glaſs ball was fixed a ſmall hollow tube or point, which projecting outwards was ſoldered to the end of a common barometer tube about 32 inches in length, and by means of this opening the ſpace between the internal ſurface of the glaſs ball and the bulb of the thermometer was filled with hot mercury, which had been previouſly freed of air and moiſture by boiling. The ball, and alſo the barometrical tube attached to it, being filled with mercury, the tube was carefully inverted, and its open end placed in a bowl in which there was a quantity of mercury. The inſtrument now became a barometer, and the mercury deſcending from the ball (which was now uppermoſt) left the ſpace ſurrounding the bulb of the thermometer free of air. The mercury having totally quitted the glaſs ball, and having ſunk in the tube to the height of 28 inches (being the height of the mercury in the common barometer at that time), with a lamp and a blow-pipe I melted the tube together, or ſealed it hermetically, about three-quarters of an inch below the ball, and cutting it at this place with a fine file, I ſeparated the ball from the long barometrical tube. The thermometer being afterwards filled with mercury in the common way, I now poſſeſſed a thermometer whoſe bulb was confined in the center of a *Torricellian vacuum*, and which ſerved at the ſame time as the body to be heated, and as the inſtrument for meaſuring the heat communicated.

Experiment N° 1.

With this inſtrument (ſee fig. 1. Tab. VI.) I made the following experiment. Having plunged it into a veſſel filled with water, warm to the 18th degree of Reaumur's ſcale, and ſuffered it to remain there till it had acquired the temperature of the water, that is to ſay, till the mercury in the incloſed thermometer ſtood at 18°, I took it out of this veſſel and plunged it ſuddenly into a veſſel of boiling water, and holding it in the water (which was kept conſtantly boiling) by the end of the tube, in ſuch a manner that the glaſs ball, in the center of which was the bulb of the thermometer, was juſt ſubmerged, I obſerved the number of degrees to which the mercury in the thermometer had ariſen at different periods of time, counted from the moment of its immerſion. Thus, after it had remained in the boiling water 1 min. 30 ſec. I found the mercury had riſen from 18° to 27°. After 4 minutes had elapſed, it had riſen to 44°$\frac{9}{10}$; and at the end of 5 minutes it had riſen to 48°$\frac{2}{10}$.

Experiment N° 2.

Taking it now out of the boiling water I ſuffered it to cool gradually in the air, and after it had acquired the temperature of the atmoſphere, which was that of 15° R. (the weather being perfectly fine), I broke off a little piece from the point of the ſmall tube which remained at the bottom of the glaſs ball, where it had been hermetically ſealed, and of courſe the atmoſpheric air ruſhed immediately into the ball. The ball ſurrounding the bulb of the thermometer being now filled with air (inſtead of being emptied of air, as it was in the before-mentioned experiment), I re-ſealed the end of the ſmall tube at the bottom of the glaſs ball hermetically, and by that means cut off all communication between the air confined in the ball and the external air; and with the inſtrument ſo prepared I repeated the experiment before-mentioned; that is to ſay, I put it into water warmed to 18°, and when it had acquired the temperature of the water, I plunged it into boiling water, and obſerved the times of the aſcent of the mercury in the thermometer. They were as follows:

	Time elapſed.	Heat acquired.
Heat at the moment of being plunged into the boiling water, . . .		18° R.
	M. S.	°
After having remained in the boiling water	0 45	27
	1 0	$34\frac{4}{10}$
	2 10	$44\frac{9}{10}$
	2 40	$48\frac{2}{10}$
	4 0	$56\frac{2}{10}$
	5 0	$60\frac{9}{10}$

From the reſult of theſe experiments it appears evidently, that the Torricellian vacuum, which affords ſo ready a paſſage to the electric fluid, ſo far from being a good conductor of heat, is a much worſe one than common air, which of itſelf is reckoned among the worſt: for in the laſt experiment, when the bulb of the thermometer was ſurrounded with air, and the inſtrument was plunged into boiling water, the mercury roſe from 18° to 27° in 45 ſeconds; but in the former experiment, when it was ſurrounded by a Torricellian vacuum, it required to remain in the boiling water 1 minute 30 ſeconds = 90 ſeconds, to acquire that degree of heat. In the vacuum it required 5 minutes to riſe to $48°\frac{2}{10}$; but in air it roſe to that height in 2 minutes 40 ſeconds; and the proportion of the times in the other obſervations is nearly the ſame, as will appear by the following table.

	The bulb of the thermometer placed in the center of the glaſs ball, and			
	ſurrounded by a Torricellian vacuum. (Exp. N° 1.)		ſurrounded by air. (Exp. N° 2.)	
	Time elapſed.	Heat acquired.	Time elapſed.	Heat acquired.
Upon being plunged into boiling water		18°		18°
	M. S.	°	M. S.	°
After remaining in it	1 30	27	0 45	27
	—	—	1 0	$30\frac{4}{10}$
	4 0	$44\frac{9}{10}$	2 10	$44\frac{9}{10}$
	5 0	$48\frac{2}{10}$	2 40	$48\frac{2}{10}$
	—	—	4 0	$56\frac{2}{10}$
	—	—	5 0	$60\frac{9}{10}$

Theſe experiments were made at Manheim, upon the firſt day of July laſt, in the preſence of Profeſſor HEMMER, of the Electoral Academy of Sciences of Manheim, and CHARLES ARTARIA, Meteorological Inſtrument-maker to the Academy, by whom I was aſſiſted.

Finding the conſtruction of the inſtrument made uſe of in theſe experiments attended with much trouble and riſque, on account of the difficulty of ſoldering the glaſs ball to the tube of the thermometer without at the ſame time either cloſing up, or otherwiſe injuring, the bore of the tube, I had recourſe to another contrivance much more commodious, and much eaſier in the execution.

At the end of a glaſs tube or cylinder ten or eleven inches in length, and near three-quarters of an inch in diameter internally, I cauſed a hollow globe to be blown 1½ inch in diameter,

meter, with an opening in the bottom of it correſponding with the bore of the tube, and equal to it in diameter, leaving to the opening a neck or ſhort tube, about an inch or three-quarters of an inch in length. Having a thermometer prepared, whoſe bulb was juſt half an inch in diameter, and whoſe freezing point fell at about 2¾ inches above its bulb, I graduated its tube according to Reaumur's ſcale, beginning at 0°, and marking that point, and alſo every tenth degree above it to 80°, with threads of fine ſilk bound round it, which being moiſtened with lac varniſh adhered firmly to the tube. This thermometer I introduced into the glaſs cylinder and globe juſt deſcribed, by the opening in the bottom of the globe, having firſt choaked the cylinder at about 2 inches from its junction with the globe by heating it, and crowding its ſides inwards towards its axis, leaving only an opening ſufficient to admit the tube of the thermometer. The thermometer being introduced into the cylinder in ſuch a manner that the center of its bulb coincided with the center of the globe, I marked a place in the cylinder, about three-quarters of an inch above the 80th degree or boiling point upon the tube of the incloſed thermometer, and taking out the thermometer, I choaked the cylinder again in this place. Introducing now the thermometer for the laſt time, I cloſed the opening at the bottom of the globe at the lamp, taking care, before I brought it to the fire, to turn the cylinder upſide down, and to let the bulb of the thermometer fall into the cylinder till it reſted upon the lower choak in the cylinder. By this means the bulb of the thermometer was removed more than 3 inches from the flame of the lamp. The opening at the bottom of the globe being now cloſed, and the bulb of the thermometer being ſuffered to return into the globe, the end of the cylinder was cut off to within about half an inch of the upper choak. This being done, it is plain, that the tube of the thermometer projected beyond the end of the cylinder. Taking hold of the end of the tube, I placed the bulb of the thermometer as nearly as poſſible in the center of the globe, and obſerving and marking a point in the tube immediately above the upper choak of the cylinder, I turned the cylinder upſide down, and ſuffering the bulb of the thermometer to enter the cylinder, and reſt upon the firſt or lower choak (by which means the end of the tube of the thermometer came further out of the cylinder), the end of the tube was cut off at the mark juſt mentioned (having firſt taken care to melt the internal cavity or bore of the tube together at that place), and a ſmall ſolid ball of glaſs, a little larger than the internal diameter or opening of the choak, was ſoldered to the end of the tube, forming a little button or knob, which reſting upon the upper choak of the cylinder, ſerved to ſuſpend the thermometer in ſuch a manner that the center of its bulb coincided with the center of the globe in which it was ſhut up. The end of the cylinder above the upper choak being now heated and drawn out to a point, or rather being formed into the figure of the fruſtum of a hollow cone, the end of it was ſoldered to the end of a barometrical tube, by the help of which the cavity of the cylinder and globe containing the thermometer was completely voided of air with mercury; when, the end of the cylinder being hermetically ſealed, the barometrical tube was detached from it with a file, and the thermometer was left completely ſhut up in a Torricellian vacuum, the center of the bulb of the thermometer being confined in the center of the glaſs globe, without touching it in any part, by means of the two choaks in the cylinder, and the button upon the end of the tube.

Of theſe inſtruments I provided myſelf with two, as nearly as poſſible of the ſame dimenſions; the one, which I ſhall call N° 1. being voided of air, in the manner above deſcribed; the other, N° 2. being filled with air, and hermetically ſealed.

With theſe two inſtruments (ſee fig. 2.) I made the following experiments upon the 11th of July laſt, at Manheim, between the hours of ten and twelve, the weather being very fine and clear, the mercury in the barometer ſtanding at 27 inches 11 lines, Reaumur's thermometer at 15°, and the quill hygrometer of the Academy of Manheim at 47°.

Experiments N° 3, 4, 5 and 6.

Putting both the inſtruments into melting ice, I let them remain there till the mercury in the incloſed thermometers reſted at the point 0°, that is to ſay, till they had acquired exactly the temperature of freezing water or melting ice; and then taking them out of the ice I plunged them ſuddenly into a large veſſel of boiling water, and obſerved the time required for the mercury to riſe in the thermometers from ten degrees to ten degrees, from 0° to 80°, taking care to keep the water conſtantly boiling during the whole of this time, and taking care alſo to keep the inſtruments immerſed to the ſame depth, that is to ſay, juſt ſo deep that the point 0° of the incloſed thermometer was even with the ſurface of the water.

Theſe experiments I repeated twice, with the utmoſt care; and the following table gives the reſult of them.

Thermometer N° 1.

Its bulb half an inch in diameter, ſhut up in the center of a hollow glaſs globe, 1½ inch in diameter, *void of air*, and hermetically ſealed.

Taken out of freezing water, and plunged into boiling water.

Time elapſed.				Heat acquired.
Exp. N° 3.		Exp. N° 4.		
				0°
M.	S.	M.	S.	°
0	51	0	51	10
0	59	0	59	20
1	1	1	2	30
1	18	1	22	40
1	24	1	23	50
2	0	1	51	60
3	30	3	6	70
11	41	10	27	80

22 44 21 1 = total time of heating from 0° to 80°.

Total time from 0° to 70°:

M. S.
In Exp. N° 3. = 11 3
In Exp. N° 4. = 10 34

Medium = 10 48¼

Thermometer N° 2.

Its bulb half an inch in diameter, ſhut up in the center of a hollow glaſs globe, 1½ inch in diameter, *filled with air*, and hermetically ſealed.

Taken out of freezing water, and plunged into boiling water.

Time elapſed.				Heat acquired.
Exp. N° 5.		Exp. N° 6.		
				0°
M.	S.	M.	S.	°
0	30	0	30	10
0	35	0	37	20
0	41	0	41	30
0	49	0	53	40
1	1	0	59	50
1	24	1	20	60
2	45	2	25	70
9	10	9	38	80

16 55 17 3 = total time of heating from 0° to 80°.

Total time from 0° to 70°:

M. S.
In Exp. N° 5. = 7 45
In Exp. N° 6. = 7 25

Medium = 7 35

It appears from theſe experiments, that the conducting power of air to that of the Torricellian vacuum, under the circumſtances deſcribed, is as $7\frac{35}{60}$ to $10\frac{48\frac{1}{4}}{60}$ inverſely, or as 1000 to 702 nearly; for the quantities of heat communicated being equal, the intenſity of the communication is as the times inverſely.

In theſe experiments the heat paſſed through the ſurrounding medium *into* the bulb of the thermometer: in order to reverſe the experiment, and make the heat paſs *out of* the thermometer, I put the inſtruments into boiling water, and let them remain therein till they had acquired the temperature of the water, that is to ſay, till the mercury in the incloſed thermometers ſtood at 80°; and then, taking them out of the boiling water, I plunged them ſuddenly into a mixture of water and pounded ice, and moving them about continually in this mixture, I obſerved the times employed in cooling as follows.

Thermometer N° 1. Surrounded by a *Torricellian vacuum*. *Taken out of boiling water, and plunged into freezing water.*			*Thermometer* N° 2. Surrounded by *air*. *Taken out of boiling water, and plunged into freezing water.*		
Time elapſed.		Heat loſt.	Time elapſed.		Heat loſt.
Exp. N° 7.	Exp. N° 8.		Exp. N° 9.	Exp. N° 10.	
		80°			80°
M. S.	M. S.		M. S.	M. S.	
1 2	0 54	70	0 33	0 33	70
0 58	1 2	60	0 39	0 34	60
1 17	1 18	50	0 44	0 44	50
1 46	1 37	40	0 55	0 55	40
2 5	2 16	30	1 17	1 18	30
3 14	3 10	20	1 57	1 57	20
5 42	5 59	10	3 44	3 40	10
Not obſerved.	Not obſerved.	0	40 10	Not obſerved.	0
Total time of cooling from 80° to 10°.			Total time of cooling from 80° to 10°.		
M. S.			M. S.		
In Exp. N° 7. = 16 4			In Exp. N° 9. = 9 49		
In Exp. N° 8. = 16 16			In Exp. N° 10. = 9 41		
Medium = 16 10			Medium = 9 45		

By theſe experiments it appears, that the conducting power of air is to that of the Torricellian vacuum as $9\frac{41}{60}$ to $16\frac{10}{60}$ inverſely, or as 1000 to 603.

To determine whether the ſame law would hold good when the heated thermometers, inſtead of being plunged into freezing water, were ſuffered to cool in the open air, I made the following experiments. The thermometers N° 1. and N° 2. being again heated in boiling water, as in the laſt experiments, I took them out of the water, and ſuſpended them in the middle of a large room, where the air (which appeared to be perfectly at reſt, the windows and doors being all ſhut) was warm to the 16th degree of Reaumur's thermometer, and the times of cooling were obſerved as follows.

(Exp. N° 11.) *Thermometer* N° 1. Surrounded by a Torricellian vacuum. *Heated to* 80°, *and ſuſpended in the open air warm to* 16°.		(Exp. N° 12.) *Thermometer* N° 2. Surrounded by air. *Heated to* 80°, *and ſuſpended in the open air warm to* 16°.	
Time elapſed.	Heat loſt.	Time elapſed.	Heat loſt.
	80°		80°
M. S.	°	M. S.	°
Not obſerved.	70	Not obſerved.	70
1 24	60	0 51	60
1 44	50	1 5	50
2 28	40	1 34	40
4 16	30	2 41	30
10 12 = total time employed in cooling from 70° to 30°.		6 11 = total time employed in cooling from 70° to 30°.	

Here the difference in the conducting powers of air and of the Torricellian vacuum appears to be nearly the ſame as in the foregoing experiments, being as $6\frac{11}{60}$ to $10\frac{12}{60}$ inverſely, or as 1000 to 605. I could not obſerve the time of cooling from

80° to 70°, being at that time buſied in ſuſpending the inſtruments.

As it might poſſibly be objected to the concluſions drawn from theſe experiments that, notwithſtanding all the care that was taken in the conſtructing of the two inſtruments made uſe of that they ſhould be perfectly alike, yet they might in reality be ſo far different, either in ſhape or ſize, as to occaſion a very ſenſible error in the reſult of the experiments; to remove theſe doubts I made the following experiments.

In the morning towards eleven o'clock, the weather being remarkably fine, the mercury in the barometer ſtanding at 27 inches 11 lines, Reaumur's thermometer at 15°, and the hygrometer at 47°, I repeated the experiment N° 3. (of heating the thermometer N° 1. in boiling water, &c.) and immediately afterwards opening the cylinder containing the thermometer at its upper end, where it had been ſealed, and letting the air into it, I re-ſealed it hermetically, and repeated the experiment again with the ſame inſtrument, the thermometer being now ſurrounded with air, like the thermometer N° 2.

The reſult of theſe experiments, which may be ſeen in the following table, ſhews evidently, that the error ariſing from the difference of the ſhapes or dimenſions of the two inſtruments in queſtion was inconſiderable, if not totally imperceptible.

(Exp. N° 13.)

Thermometer N° 1.

Its bulb half an inch in diameter ſhut up in the center of a glaſs globe 1½ inch in diameter, *voided of air*, and hermetically ſealed.

Taken out of freezing water, and plunged into boiling water.

Time elapſed.		Heat acquired.
—		0°
M.	S.	°
0	55	10
0	55	20
1	7	30
1	15	40
1	29	50
2	2	60
3	21	70
13	44	80

24 48 = total time of heating from 0° to 80°.

Total time from 0° to 70° = 11′ 4″.

(Exp. N° 14.)

The ſame Thermometer (N° 1.)

The glaſs globe, containing the bulb of the thermometer, being now *filled with air*, and hermetically ſealed.

Taken out of freezing water, and plunged into boiling water.

Time elapſed.		Heat acquired.
—		0°
M.	S.	°
0	32	10
0	32	20
0	43	30
0	50	40
1	1	50
1	24	60
2	38	70
10	25	80

18 5 = total time of heating from 0° to 80°.

Total time from 0° to 70° = 7′ 40″.

It appears, therefore, from theſe experiments, that the conducting power of common atmoſpheric air is to that of the Torricellian vacuum as $7\frac{40}{60}$ to $11\frac{4}{60}$ inverſely, or as 1000 to 602; which differs but very little from the reſult of all the foregoing experiments.

Notwithſtanding that it appeared, from the reſult of theſe laſt experiments, that any difference there might poſſibly have been in the proportions or dimenſions of the inſtruments N° 1. and N° 2. could hardly have produced any ſenſible error in the reſult of the experiments in queſtion; I was willing, however, to ſee how far any conſiderable alterations of ſize in the inſtrument would affect the experiment: I therefore provided myſelf with another inſtrument, which I ſhall call *Thermometer* N° 3. different from thoſe already deſcribed in ſize, and a little different in its conſtruction.

The bulb of the thermometer was of the ſame form and ſize as in the inſtruments N° 1. and N° 2. that is to ſay, it was globular, and half an inch in diameter; but the glaſs globe, in the center of which it was confined, was much larger, being 3 inches 7½ lines in diameter; and the bore of the tube of the thermometer was much finer, and conſequently its length, and the diviſions of its ſcale, were greater. The diviſions were marked upon the tube with threads of ſilk of different colours at every tenth degree, from 0° to 80°, as in the before-mentioned inſtruments. The tube or cylinder belonging to the glaſs globe was 8 lines in diameter, a little longer than the tube of the thermometer, and perfectly cylindrical from its upper end to its junction with the globe, being without any choak; the thermometer being confined in the center of the globe by a different contrivance, which was as follows. To the opening of the cylinder was fitted a ſtopple of dry wood, covered with a coat of hard varniſh, through the center or axis of which paſſed the end of the tube of the thermometer: this confined the tube in the axis of the cylinder at its upper end. To confine it at its lower end, there was fitted to it a ſmall ſteel ſpring, a little below the point 0°; which, being confined round the tube of the thermometer, had three elaſtic points projecting outwards, which preſſing againſt the inſide of the cylinder, confined the thermometer in its place. The total length of this inſtrument, from the bottom of the globe to the upper end of the cylinder, was 18 inches, and the freezing point upon the thermometer fell about 3 inches above the bulb; conſequently it lay about 1½ inch above the junction of the cylinder with the globe, when the thermometer was confined in its place, the center of its bulb coinciding with the center of the globe. Through the ſtopple which cloſed the end of the cylinder paſſed two ſmall glaſs tubes, about a line in diameter, which being about a line longer than the ſtopple were ſtopped up occaſionally with ſmall ſtopples fitted to their bores. Theſe tubes (which were fitted exactly in the holes bored in the great ſtopple of the cylinder to receive them, and fixed in their places with cement) ſerved to convey air, or any other fluid, into the glaſs ball, without being under a neceſſity of removing the ſtopple cloſing the end of the cylinder; which, in order to prevent the poſition of the thermometer from being eaſily deranged, was cemented in its place.

I have been the more particular in the deſcription of theſe inſtruments, as I conceive it abſolutely neceſſary to have a perfect idea of them in order to judge of the experiments made with them.

With the inſtrument laſt deſcribed (which I have called *Thermometer* N° 3.) I made the following experiment. It was upon the 18th of July laſt, in the afternoon, the weather variable, alternate clouds and ſun-ſhine; wind ſtrong at S.E. with now and then a ſprinkling of rain; barometer at 27 inches 10½ lines, thermometer at 18°¼, and hygrometer variable from 44° to extreme moiſture.

In order to compare the reſult of the experiment made with this thermometer with thoſe made with the thermometer N° 2. I have, in the following table, placed theſe experiments by the ſide of each other.

(Exp. N° 15.)
Thermometer N° 3.
Its bulb half an inch in diameter, ſhut up in the center of a glaſs tube, 3 inches 7½ lines in diameter, and ſurrounded by *air*.
Taken out of freezing water, and plunged into boiling water.

Time elapſed.		Heat acquired.
M.	S.	0°
		0
0	33	10
0	38	20
0	54	30
0	51	40
1	7	50
1	28	60
2	28	70
9	0	80

16 59 = total time of heating from 0° to 80°.
Time from 0° to 70° = 7′ 59″.

(Exp. N° 4. and N° 5.)
Thermometer N° 2.
Its bulb half an inch in diameter, ſhut up in the center of a glaſs globe, 1½ inch in diameter, and ſurrounded by *air*.
Taken out of freezing water, and plunged into boiling water.

Time elapſed.						Heat acquired.
Exp. N° 4.		Exp. N° 5.		Medium.		
M.	S.	M.	S.	M.	S.	0°
						0
0	30	0	30	0	30	10
0	35	0	37	0	36	20
0	41	0	41	0	41	30
0	49	0	53	0	51	40
1	1	0	59	1	0	50
1	24	1	20	1	22	60
2	45	2	25	2	35	70
9	10	9	38	9	24	80
16	55	17	3	16	59	= total

time of heating from 0° to 80°.
Time from 0° to 70° = 7′ 35″

If the agreement of theſe experiments with the thermometers N° 2. and N° 3. ſurpriſed me, I was not leſs ſurpriſed with their diſagreement in the experiment which follows.

Experiment N° 16.

Taking the thermometer N° 3. out of the boiling water, I immediately ſuſpended it in the middle of a large room, where the air, which was quiet, had the temperature of 18°¼ R. and obſerved the times of cooling as follows:

Time

Time elapſed.		Heat loſt.
		80°
M.	S.	°
1	55	70
0	12	60
0	33	50
2	15	40
4	0	30

9 55 = total time of cooling from 80° to 30°.

Time from 70° to 30° = 8′ 0″; but in the experiment N° 12. with the thermometer N° 2. the time employed in cooling from 70° to 30° was only 6′ 11″. In this experiment, with the thermometer N° 3. the time employed in cooling from 60° to 30° was 7′ 48″; but in the above-mentioned experiment, with the thermometer N° 2. it was only 5′ 20″. It is true, the air of the room was ſomewhat cooler when the former experiment was made than when this latter was made with the thermometer N° 3.; but this difference of temperature, which was only 2°½ (in the former caſe the thermometer in the room ſtanding at 16°, and in the latter at 18°½) certainly could not have occaſioned the whole of the apparent difference in the reſults of the experiments.

Does air receive heat more readily than it parts with it? This is a queſtion highly deſerving of further inveſtigation, and I ſhall not fail to give it a full examination in the courſe of my projected inquiries; but leaving it for the preſent, I ſhall proceed to give an account of the experiments which I have already made *.

It

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* Conceiving it to be a ſtep of conſiderable importance towards coming at a further

It having been my intention from the beginning to examine the conducting powers of the artificial airs or gaſſes, the thermometer

further knowledge of the nature of heat, to aſcertain, by indiſputable evidence, its paſſage through the Torricellian vacuum, and to determine, with as much preciſion as poſſible, the law of its motions in that medium; and being apprehenſive that doubts might ariſe with reſpect to the experiments before deſcribed, on account of the contact of the tubes of the incloſed thermometers in the inſtruments made uſe of with the containing glaſs globes, or rather with their cylinders; by which means it might be ſuſpected, that a certain quantity, if not all the heat acquired, might poſſibly be communicated: to put this matter beyond all doubt, I made the following experiment.

In the middle of a glaſs body, of a pear-like form, about 8 inches long, and 2½ inches in its greateſt diameter, I ſuſpended a ſmall mercurial thermometer, 5½ inches long, by a fine thread of ſilk, in ſuch a manner that neither the bulb of the thermometer, nor its tube, touched the containing glaſs body in any part. The tube of the thermometer was graduated, and marked with fine threads of ſilk of different colours bound round it, as in the thermometers belonging to the other inſtruments already deſcribed; and the thermometer was ſuſpended in its place by means of a ſmall ſteel ſpring, to which the end of the thread of ſilk which held the thermometer being attached, it (the ſpring) was forced into a ſmall globular protuberance or cavity, blown in the upper extremity of the glaſs body, about half an inch in diameter, where the ſpring remaining, the thermometer neceſſarily remained ſuſpended in the axis of the glaſs body. There was an opening at the bottom of the glaſs body, through which the thermometer was introduced; and a barometrical tube being ſoldered to this opening, the inſide of the glaſs body was voided of air by means of mercury; and this opening being afterwards ſealed hermetically, and the barometrical tube being taken away, the thermometer was left ſuſpended in a Torricellian vacuum.

In this inſtrument, as the incloſed thermometer did not touch the containing glaſs body in any part, on the contrary, being diſtant from its internal ſurface an inch or more in every part, it is clear, that whatever heat paſſed *into* or *out of* the thermometer muſt have paſſed *through* the ſurrounding Torricellian vacuum: for it cannot be ſuppoſed, that the fine thread of ſilk, by which the thermometer was ſuſpended, was capable of conducting any heat at all, or at leaſt any ſenſible quantity. I therefore flattered myſelf with hopes of being able, with the aſſiſtance thermometer N° 3. was conſtructed with a view to thoſe experiments; and having now provided myſelf with a ſtock of thoſe different kinds of airs, I began with *fixed air*, with which, by

aſſiſtance of this inſtrument, to determine poſitively with regard to the paſſage of heat in the Torricellian vacuum: and this, I think, I have done, notwithſtanding that an unfortunate accident put it out of my power to purſue the experiments ſo far as I intended.

This inſtrument being fitted to a ſmall ſtand or foot of wood, in ſuch a manner that the glaſs body remained in a perpendicular ſituation, I placed it in my room, by the ſide of another incloſed thermometer (N° 2.), which was ſurrounded by air, and obſerved the effect of the variation of heat in the atmoſphere. I ſoon diſcovered, by the motion of the mercury in the incloſed thermometer, that the heat paſſed through the Torricellian vacuum; but it appeared plainly from the ſluggiſhneſs or great inſenſibility of the thermometer, that the heat paſſed with much greater difficulty in this medium than in common air. I now plunged both the thermometers into a bucket of cold water; and I obſerved that the mercury in the thermometer ſurrounded by air deſcended much faſter than that in the thermometer ſurrounded by the Torricellian vacuum. I took them out of the cold water, and plunged them into a veſſel of hot water (having no conveniencies at hand to repeat the experiment in form with the freezing and with the boiling water); and the thermometer ſurrounded by the Torricellian vacuum appeared ſtill to be much more inſenſible or ſluggiſh than that ſurrounded by air.

Theſe trials were quite ſufficient to convince me of the paſſage of heat in the Torricellian vacuum, and alſo of the greater difficulty of its paſſage in that medium than in common air; but, not ſatisfied to reſt my inquiries here, I took the firſt opportunity that offered, and ſet myſelf to repeat the experiments which I had before made with the inſtruments N° 1. and N° 2. I plunged this inſtrument into freezing water, where I let it remain till the mercury in the incloſed thermometer had deſcended to 0°; when, taking it out of the freezing water, I plunged it ſuddenly into a veſſel of boiling water, and prepared myſelf to obſerve the aſcent of the mercury in the incloſed thermometer as in the foregoing experiments; but unfortunately the moment the end of the glaſs body touched the boiling water, it cracked with the heat at the point where it had been hermetically ſealed, and the water ruſhing into the body, ſpoiled the experiment: and I have not ſince had an opportunity of providing myſelf with another inſtrument to repeat it.

means of water, I filled the globe and cylinder containing the thermometer; and ſtopping up the two holes in the great ſtopple cloſing the end of the cylinder, I expoſed the inſtrument in freezing water till the mercury in the incloſed thermometer had deſcended to 0°; when, taking it out of the freezing water, I plunged it into a large veſſel of boiling water, and prepared myſelf to obſerve the times of heating, as in the former caſes; but an accident happened, which ſuddenly put a ſtop to the experiment. Immediately upon plunging the inſtrument into the boiling water, the mercury began to riſe in the thermometer with ſuch uncommon celerity, that it had paſſed the firſt diviſion upon the tube (which marked the 10th degree, according to REAUMUR's ſcale) before I was aware of its being yet in motion; and having thus miſſed the opportunity of obſerving the time elapſed when the mercury arrived at that point, I was preparing to obſerve its paſſage of the next, when all of a ſudden the ſtopple cloſing the end of the cylinder was blown up the chimney with a great exploſion, and the thermometer, which, being cemented to it by its tube, was taken along with it, and was broken to pieces, and deſtroyed in its fall.

This unfortunate experiment, though it put a ſtop for the time to the inquiries propoſed, opened the way to other reſearches not leſs intereſting. Suſpecting that the exploſion was occaſioned by the rarefaction of the water which remained attached to the inſide of the globe and cylinder after the operation of filling them with fixed air; and thinking it more than probable, that the uncommon celerity, with which the mercury roſe in the thermometer, was principally owing to the ſame cauſe; I was led to examine the conducting power of *moiſt air*, or air ſaturated with water.

For this experiment I provided myſelf with a new thermometer N° 4. the bulb of which, being of the ſame form as thoſe already deſcribed (*viz.* globular) was alſo of the ſame ſize, or half an inch in diameter. To receive this thermometer a glaſs cylinder was provided, 8 lines in diameter, and about 14 inches long, and terminated at one end by a globe 1½ inch in diameter. In the center of this globe the bulb of the thermometer was confined, by means of the ſtopple which cloſed the end of the cylinder; which ſtopple, being near 2 inches long, received the end of the tube of the thermometer into a hole bored through its center or axis, and confined the thermometer in its place, without the aſſiſtance of any other apparatus. Through this ſtopple two other ſmall holes were bored, and lined with thin glaſs tubes, as in the thermometer N° 3. opening a paſſage into the cylinder, which holes were occaſionally ſtopped up with ſome ſtopples of cork; but to prevent accidents, ſuch as I had before experienced from an exploſion, great care was taken not to preſs theſe ſtopples into their places with any conſiderable force, that they might the more eaſily be blown out by any conſiderable effort of the confined air.

Though in this inſtrument the thermometer was not altogether ſo ſteady in its place as in the thermometers N° 1. N° 2. and N° 3. the elaſticity of the tube, and the weight of the mercury in the bulb of the thermometer, occaſioning a ſmall vibration or trembling of the thermometer upon any ſudden motion or jar; yet I preferred this method to the others, on account of the lower part of this thermometer being intirely free, or ſuſpended in ſuch a manner as not to touch, or have any communication with, the lower part of the cylinder or the globe: for though the quantity of heat received by the tube of the thermometer at its contact with the cylinder at its choaks, in the inſtruments N° 1. and N° 2. or with the branches of the ſteel ſpring in N° 3. and from thence communicated to the bulb, muſt have been exceedingly ſmall; yet I was deſirous to prevent even that, and every other poſſible error or inaccuracy, however ſmall, that might ariſe.

Does humidity augment the conducting power of air?

To determine this queſtion I made the following experiments, the weather being clear and fine, the mercury in the barometer ſtanding at 27 inches 8 lines, the thermometer at 19°, and the hygrometer at 44°.

(Exp. N° 17.) *Thermometer* N° 4.		(Exp. N° 18.) *The ſame thermometer* (N° 4.)	
Surrounded by air *dry* to the 44th degree of the quill hygrometer of the Manheim Academy. *Taken out of freezing water, and plunged into boiling water.*		Surrounded by air rendered as *moiſt* as poſſible by wetting the inſide of the cylinder and globe with water. *Taken out of freezing water, and plunged into boiling water.*	
Time elapſed.	Heat acquired.	Time elapſed.	Heat acquired.
	0°		0°
M. S.	°	M. S.	°
0 34	10	0 6	10
0 39	20	0 4	20
0 44	30	0 5	30
0 51	40	0 9	40
1 6	50	0 18	50
1 35	60	0 26	60
2 40	70	0 43	70
not obſerved.	80	7 45	80
8 9 = total time of heating from 0° to 70°.		1 51 = total time of heating from 0° to 70°.	

From theſe experiments it appears, that the conducting power of air is very much increaſed by humidity. To ſee if the ſame reſult would obtain when the experiment was reverſed, I now took the thermometer with the *moiſt air* out of the boiling water, and plunged it into freezing water; and moving it about continually from place to place in the freezing water, I obſerved the times of cooling, as ſet down in the following table. N. B. To compare the reſult of this experiment with thoſe made with *dry air*, I have placed on one ſide in the following table the experiment in queſtion, and on the other ſide the experiment N° 19. made with the thermometer N° 2.

(Exp. N° 19.) *Thermometer* N° 4. Surrounded by *moiſt air.* *Taken out of boiling water, and plunged into freezing water.*		(Exp. N° 10.) *Thermometer* N° 2. Surrounded by *dry air.* *Taken out of boiling water, and plunged into freezing water.*	
Time elapſed.	Heat loſt.	Time elapſed.	Heat loſt.
	80°		80°
M. S.	°	M. S.	°
0 4	70	0 33	70
0 14	60	0 34	60
0 31	50	0 44	50
0 52	40	0 55	40
1 22	30	1 18	30
2 3	20	1 57	20
4 2	10	3 40	10
9 8 = total time of cooling from 80° to 10°.		9 12 = total time of cooling from 80° to 10°.	

Though the difference of the whole times of cooling from 80° to 10° in theſe two experiments appears to have been very ſmall, yet the difference of the times taken up by the firſt twenty or thirty degrees from the boiling point is very remarkable, and ſhows with how much greater facility heat paſſes in moiſt air than in dry air. Even the ſlowneſs with which the mercury in the thermometer N° 4. deſcended in this experiment from the 30th to the 20th, and from the 20th to the 10th degree, I attribute in ſome meaſure to the great conducting power of the moiſt air with which it was ſurrounded; for the cylinder containing the thermometer and the moiſt air, being not wholly ſubmerged in the freezing water, that part of it which remained out of the water was neceſſarily ſurrounded by the air of the atmoſphere; which being much warmer than the water, communicated of its heat to the glaſs; which, paſſing from thence into the contained moiſt air as ſoon as that air became colder than the external air, was, through that medium, communicated to the bulb of the incloſed thermometer, which prevented its cooling ſo faſt as it would otherwiſe have done. But when the weather becomes cold, I propoſe to repeat this experiment with variations, in ſuch a manner as to put the matter beyond all doubt. In the mean time I cannot help obſerving, with what infinite wiſdom and goodneſs Divine Providence appears to have guarded us againſt the evil effects of exceſſive heat and cold in the atmoſphere; for if it were poſſible for the air to be equally damp during the ſevere cold of the winter months as it ſometimes is in ſummer, its conducting power, and conſequently its apparent coldneſs, when applied to our bodies, would be ſo much increaſed, by ſuch an additional degree of moiſture, that it would become quite intolerable; but, happily for us, its power to hold water in ſolution is diminiſhed, and with it its power to rob us of our animal heat, in proportion as its coldneſs is increaſed. Every body knows how very diſagreeable a very moderate degree of cold is when the air is very damp; and from hence it appears, why the thermometer is not always a juſt meaſure of the apparent or ſenſible heat of the atmoſphere. If colds or catarrhs are occaſioned by our bodies being robbed of our animal heat, the reaſon is plain why thoſe diſorders prevail moſt during the cold autumnal rains, and upon the breaking up of the froſt in the ſpring. It is likewiſe plain from whence it is that ſleeping in damp beds, and inhabiting damp houſes, is ſo very dangerous; and why the evening air is ſo pernicious in ſummer and in autumn, and why it is not ſo during the hard froſts of winter. It has puzzled many very able philoſophers and phyſicians to account for the manner in which the extraordinary degree or rather *quantity* of heat is generated which an animal body is ſuppoſed to loſe, when expoſed to the cold of winter, above what it communicates to the ſurrounding atmoſphere in warm ſummer weather; but is it not more than probable, that the difference of the quantities of heat, actually loſt or communicated, is infinitely leſs than what they have imagined? Theſe inquiries are certainly very intereſting; and they are undoubtedly within the reach of well contrived and well conducted experiments. But taking my leave for the preſent of this curious ſubject of inveſtigation, I haſten to the ſequel of my experiments.

Finding ſo great a difference in the conducting powers of common air and of the Torricellian vacuum, I was led to examine the conducting powers of common air of different degrees of denſity. For this experiment I prepared the thermometer N° 4. by ſtopping up one of the ſmall glaſs tubes paſſing through the ſtopple, and opening a paſſage into the cylinder, and by fitting a valve to the external overture of the other. The inſtrument, thus prepared, being put under the receiver of an air-pump, the air paſſed freely out of the globe and cylinder upon working the machine, but the valve above deſcribed prevented its return upon letting air into the receiver. The gage of the air-pump ſhowed the degree of rarity of the air under the receiver, and conſequently of that filling the globe and cylinder, and immediately ſurrounding the thermometer.

With this inſtrument, the weather being clear and fine, the mercury in the barometer ſtanding at 27 inches 9 lines, the thermometer at 15°, and the hygrometer at 47°, I made the following experiments.

(Exp. Nº 20.) *Thermometer* Nº 4. Surrounded by common air, barometer ſtanding at 27 inches 9 lines. *Taken out of freezing water, and plunged into boiling water.*		(Exp. Nº 21.) *Thermometer* Nº 4. Surrounded by air rarefied by pumping till the barometer-gage ſtood at 6 inches 11½ lines. *Taken out of freezing water, and plunged into boiling water.*		(Exp. Nº 22.) *Thermometer* Nº 4. Surrounded by air rarefied by pumping till the barometer-gage ſtood at 1 inch 2 lines. *Taken out of freezing water, and plunged into boiling water.*	
Time elapſed.	Heat acquired.	Time elapſed.	Heat acquired.	Time elapſed.	Heat acquired.
	0°		0°		0°
M. S.	°	M. S.	°	M. S.	°
0 31	10	0 31	10	0 29	10
0 40	20	0 38	20	0 36	20
0 41	30	0 44	30	0 49	30
0 47	40	0 51	40	1 1	40
1 4	50	1 7	50	1 1	50
1 25	60	1 19	60	1 24	60
2 28	70	2 27	70	2 31	70
10 17	80	10 21	80	not obſerved.	80
7 36 = total time of heating from 0° to 70°.		7 37 = total time of heating from 0° to 70°.		7 51 = total time of heating from 0° to 70°.	

The reſult of theſe experiments, I confeſs, ſurpriſed me not a little; but the diſcovery of truth being the ſole object of my inquiries (having no favourite theory to defend) it brings no diſappointment along with it, under whatever unexpected ſhape it may appear. I hope that further experiments may lead to the diſcovery of the cauſe why there is ſo little difference in the conducting powers of air of ſuch very different degrees of rarity, while there is ſo great a difference in the conducting powers of air, and of the Torricellian vacuum. At preſent, I ſhall not venture any conjectures upon the ſubject; but in the mean time I dare to aſſert, that the experiments I have made may be depended on.

The time of my ſtay at Manheim being expired (having had the honour to attend thither his moſt Serene Highneſs the Elector Palatine Duke of Bavaria, my moſt Gracious Maſter, in his late journey), I was prevented from purſuing theſe inquiries further at that time; but I ſhall not fail to recommence them the firſt leiſure time I can find, which I fancy will be about the beginning of the month of November. In the mean time, to enable myſelf to purſue them with effect, I am ſparing neither labour nor expence to provide a complete apparatus neceſſary for my purpoſe; and his Electoral Highneſs has been graciouſly pleaſed to order M. ARTARIA (who is in his ſervice) to come to Munich to aſſiſt me. With ſuch a Patron as his moſt Serene Highneſs, and with ſuch an aſſiſtant as ARTARIA, I ſhall go on in my purſuits with chearfulneſs. Would to God that my labours might be as uſeful to others as they will be pleaſant to me!

I ſhall conclude this letter with a ſhort account of ſome experiments I have made to determine the conducting powers of water and of mercury; and with a table, ſhowing at one view the conducting powers of all the different mediums which I have examined.

Having filled the glaſs globe incloſing the bulb of the thermometer N° 4. firſt with water, and then with mercury, I made the following experiments, to aſcertain the conducting powers of thoſe two fluids.

(Exp. N° 23.) *Thermometer* N° 4. Surrounded by *water*. *Taken out of freezing water, and plunged into boiling water.*			(Exp. N° 24, 25, and 26.) *Thermometer* N° 4. Surrounded by *mercury*. *Taken out of freezing water, and plunged into boiling water.*						
Time elapſed.		Heat acquired.	Time elapſed.						Heat acquired.
			Exp. N° 24.		Exp. N° 25.		Exp. N° 26.		
		0°							0°
M.	S.		M.	S.	M.	S.	M.	S.	
0	19	10	0	5	0	5	0	5	10
0	8	20	0	4	0	2	0	5	20
0	9	30	0	2	0	2	0	4	30
0	11	40	0	4	0	5	0	5	40
0	15	50	0	4	0	4	0	7	50
0	21	60	0	7	0	4	0	8	60
0	34	70	0	15	0	9	0	14	70
2	13	80	Not obſerved.		0	58	Not obſerved.		80
1	57 = total time of heating from 0° to 70°.		0	41	0	31	0	48 = total times of heating from 0° to 70°.	

The total times of heating from 0° to 70° in the three experiments with mercury being 41 ſeconds, 31 ſeconds, and 48 ſeconds, the mean of theſe times is 36⅔ ſeconds; and as in the experiment with water the time employed in acquiring the ſame degree of heat was 1′ 57″ = 117 ſeconds, it appears from theſe experiments, that the conducting power of mercury to that of water, under the circumſtances deſcribed, is as 36⅔ to 117 inverſely, or as 1000 to 313. And hence it is plain, why mercury *appears* ſo much hotter, and ſo much colder, to the touch than water, when in fact it is of the ſame temperature: for the force or violence of the ſenſation of *hot* or *cold* depends not intirely upon the temperature of the body exciting in us thoſe ſenſations, or upon the degree of heat it actually poſſeſſes, but upon the *quantity* of heat it is capable of communicating to us, or receiving from us, in any given ſhort period of time, or as the intenſity of the communication; and this depends in a great meaſure upon the conducting powers of the bodies in queſtion.

The ſenſation of *hot* is the entrance of heat into our bodies; that of *cold* is its exit; and whatever contributes to facilitate or accelerate this communication adds to the violence of the ſenſation. And this is another proof that the thermometer cannot be a juſt meaſure of the *ſenſible* heat, or cold, exiſting in bodies; or rather, that the touch does not afford us a juſt indication of their *real* temperatures.

A Table

Fig. 1.

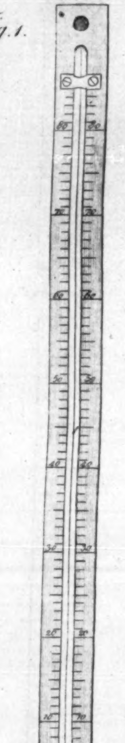


Fig. 2.

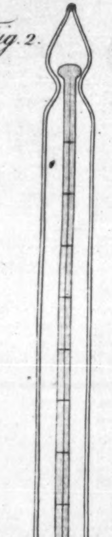


Fig. 3.

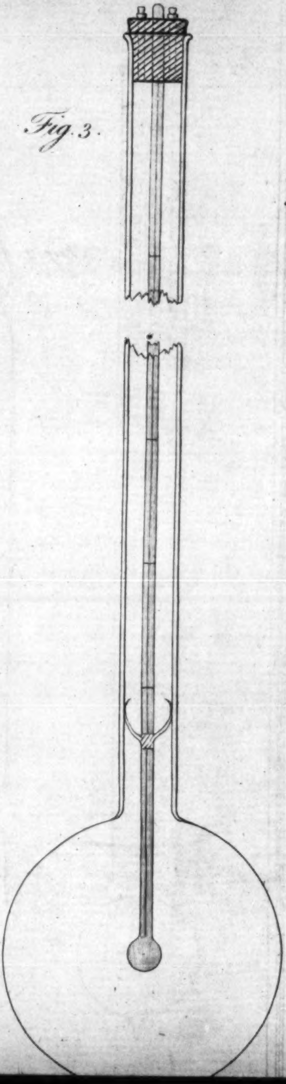
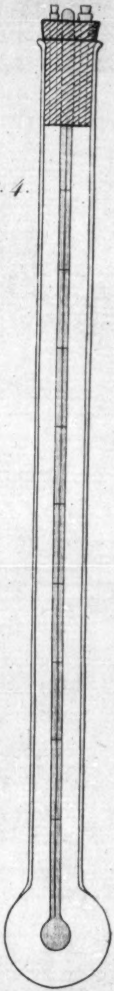


Fig. 4.



A Table of the *conducting Powers* of the under-mentioned *Mediums*, as determined by the foregoing experiments.

Thermom. N° 1.	Thermometer N° 4.						
Taken out of freezing water, and plunged into boiling water.							
Time elapſed.							
Torricellian Vacuum (Exp. N° 3. 4. and 13.)	Common air, denſity = 1, (Exp. N° 20.)	Rarefied air, denſity = $\frac{1}{4}$ (Exp. N° 21.)	Rarefied air, denſity = $\frac{1}{24}$ (Exp. N° 22.)	Moiſt air (Exp. N° 18.)	Water (Exp. N° 23.)	Mercury (Exp. N° 24, 25, and 26.)	Heat acquired.
—	—	—	—	—	—	—	0°
M. S.	M. S.	M. S.	M. S.	M. S.	M. S.	M. S.	
0 52	0 31	0 31	0 29	0 6	0 19	0 5	10°
0 58	0 40	0 38	0 36	0 4	0 8	0 3⅔	20
1 3	0 41	0 44	0 49	0 5	0 9	0 2⅔	30
1 18	0 47	0 51	1 1	0 9	0 11	0 4⅔	40
1 25	1 4	1 7	1 1	0 18	0 15	0 5	50
1 58	1 25	1 19	1 24	0 26	0 21	0 6⅓	60
3 19	2 28	2 27	2 31	0 43	0 34	0 12⅔	70
11 57	10 17	10 21	—	7 45	2 13	0 58	80
10 53	7 36	7 37	7 51	1 51	1 57	0 36⅔	= total times of heating from 0° to 70°.

In determining the relative conducting powers of theſe mediums, I have compared the times of the heating of the thermometers from 0° to 70° inſtead of taking the whole times from 0° to 80°, on account of the ſmall variation in the heat of the boiling water ariſing from the variation of the weight of the atmoſphere, and alſo on account of the very ſlow motion of the mercury between the 70th and the 80th degrees, and the difficulty of determining the preciſe moment when the mercury arrives at the 80th degree.

Taking now the conducting power of mercury = 1000, the conducting powers of the other mediums, as determined by theſe experiments, will be as follows, *viz.*

Mercury . .	1000
Moiſt air . .	330
Water, . .	313
Common air, denſity = 1	$80\frac{41}{100}$
Rarefied air, denſity = $\frac{1}{4}$.	$80\frac{23}{100}$
Rarefied air, denſity = $\frac{1}{24}$	78
The Torricellian vacuum	55

And in theſe proportions are the quantities of heat which theſe different mediums are capable of tranſmitting in any given time; and conſequently theſe numbers expreſs the relative *ſenſible* temperatures of the mediums, as well as their conducting powers. How far theſe deciſions will hold good under a variation of circumſtances experiment only can determine. This is certainly a ſubject of inveſtigation not leſs curious in itſelf than it is intereſting to mankind; and I wiſh that what I have done may induce others to turn their attention to this long neglected field of experimental inquiry. For my own part, I am determined not to quit it.

In the future proſecution of theſe inquiries, I do not mean to confine myſelf ſolely to the determining of the conducting powers of fluids; on the contrary, ſolids, and particularly ſuch bodies as are made uſe of for cloathing, will be principal ſubjects of my future experiments. I have indeed already begun theſe reſearches, and have made ſome progreſs in them; but I forbear to anticipate a matter which I propoſe for the ſubject of a future communication.

◆

XV. *Hiſtory and Diſſection of an extraordinary Introſuſception.* *By* John Coakley Lettſom, *M. D. F. R. S. and A. S.*

Read March 16, 1786.

A. B. a child four years old, was firſt indiſpoſed about the middle of September, 1784. When I was conſulted, which was on the 7th of October, the ſymptoms reſembled thoſe of a cholera morbus. At this period, however, the diarrhœa had ceaſed; but the patient continued frequently to vomit, eſpecially after taking nouriſhment.

On the 20th a dyſentery ſucceeded, with mucous and bloody ſtools, which ceaſed after a few days continuance, when ſhe nearly recovered her former ſtate of health, without even reaching after her uſual food. She was now removed into the country; and I did not hear of her again till December, when ſhe was brought to town, on account of the return of the dyſentery, which was, at this period, accompanied with a troubleſome teneſmus, and a conſiderable degree of fever.

By anodyne medicines, and the uſe of opiate clyſters, theſe complaints were occaſionally moderated, and ſometimes they totally ceaſed for a few days, but ſeldom longer, and the intervals of relief gradually ſhortened; the attacks became alſo more violent, commencing with violent rigors, and fever ſucceeding; the pulſe grew weaker and weaker, and the patient became extremely extenuated in fleſh; and towards the concluſion of this month, after repeated vomitings of a dark-coloured fluid, like coffee grounds, it finiſhed its painful exiſtence.

Bleeding, before the debility was become alarming, afforded no material reſpite. Fomentations to the abdomen, and tepid bathing of the whole body, were equally ineffectual. Anodyne ſtarch clyſters afforded ſome truce, but it could not be durable; the nature of the miſchief was too momentous to afford any hope of permanent relief, as the diſſection after death will evince.

Examination of the Body after Death, by Mr. THOMAS WHATELY, *Surgeon.*

Upon expoſing the cavity of the abdomen, the ſigmoid flexure of the colon immediately preſented itſelf to view, enlarged to the ſize of that of an adult, as alſo a large diſtended inteſtine appearing to be at firſt view a continuation of the tranſverſe arch of this gut; and at the place where this ſeeming arch joined the ſigmoid flexure, there appeared a firm or tight band, as if ſurrounding the inteſtine, and here it was ſtrongly bound down.

On a nicer inſpection this arch was found to be a portion of the ileum, which paſſing within the band was incloſed in the ſigmoid flexure of the colon.

All the parts between this portion of the ſmall inteſtines and the ſigmoid flexure, amongſt which were the caput coli, cæcum with its appendix, and the whole of the great arch of the colon, could no where be ſeen. The want of theſe parts, the enlarged ſize of the ſigmoid flexure, and the hard feel evidently ſhewing that it contained ſome ſubſtance, left no room to doubt, but that all the miſſing portion of the inteſtines was contained within the ſigmoid flexure. A finger introduced into the anus felt a round ſubſtance in the rectum, with an opening in the middle, not unlike the os tincæ. This ſubſtance did not adhere, the finger paſſing round it freely, between it and the internal coat of the rectum. The liver, the urinary bladder, and ſmall inteſtines, were the remaining parts which firſt appeared when the parietes of the abdomen were turned back.

Upon looking for the omentum, a portion of it only was found attached to the ſtomach, the remaining part evidently paſſed within the band into the ſigmoid flexure.

The ſtomach was tied much cloſer to the ſpine than natural, by the diſplacing of the omentum and great arch of the colon. The gall bladder was as large as that of an adult, and was full of thin bile, but without obſtruction to its paſſage into the duodenum.

The general external appearance of all the inteſtines was natural, except ſlight inflammation in ſome places.

The cavity of the abdomen alſo contained more than half an ounce of thin pus; and on the right ſide were two ligamentous peritoneal ſubſtances, very much on the ſtretch; one formed by an extenſion of that part of the peritoneum called ligamentum * coli dextrum; the other at the place where the colon is connected to the peritoneum over the right kidney.

As the further inveſtigation of this uncommon diſeaſe required particular attention, I cut out all the parts connected with it, bringing away the whole ſigmoid flexure of the colon,

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* I have obſerved, that in ſome children the caput coli is naturally connected much more looſely than in others. It is probable, that the preſent caſe was one of thoſe.

with the rectum, anus, uterus, and bladder; alſo a part of the arch of the ileum with the omentum, and a portion of the ſtomach and duodenum.

The Drawing * (Tab. VII.) was taken by Mr. Pole, Surgeon, of the natural ſize, and the ſmall inteſtines added from a ſketch I made before the parts were removed from the body.

I then made a longitudinal inciſion through the coats of the ſigmoid flexure of the colon, from the anus to the band at its upper part. Within the cavity, which was lined with mucus, appeared a large inteſtine, taking on the form of the ſigmoid flexure, which on examination proved to be the great arch of the colon and the cæcum inverted; ſo that the villous coat was external, and in contact with the villous coat of the ſigmoid flexure, through the whole extent of both; at the extremity of which inverted inteſtine were two apertures, viz. the large one felt by the finger *per anum*, and a ſmaller one.

It now plainly appeared, that the band was formed by the upper part of the ſigmoid flexure being drawn tight by the inverſion of the part of the colon immediately above it, the further progreſs of which was prevented by the peritoneal connections at that place not giving way; which cauſed it to appear as a band tying the inteſtine down.

This incloſed inteſtine was very much diſeaſed, the upper part next the band being highly inflamed, and as it approached the caput coli in the rectum gradually terminated in mortification, ſo that for three inches from its extremity it was perfectly black.

No adheſion whatever appeared between the coats of theſe inteſtines, as this inverted colon might be lifted out of the ſigmoid flexure to the band.

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* Mr. Basire very accurately reduced the ſcale under my own inſpection from which the engravings are taken.

Upon cutting through the coats of this inverted inteſtine it was obſerved, that they were very much thickened and diſeaſed; the enlargement of the gut, which was fully equal to that of an adult, conſiſting chiefly in a thickening of its various muſcular fibres *. The peritoneal coat, now become its internal ſurface, was every where highly inflamed, but not black as on the outſide, the inflammation gradually increaſing from the band to the extremity of the cæcum. Through the whole length of its cavity was included a portion of the ileum uninverted, with its connecting meſentery, which communicated with the larger aperture above deſcribed at the extremity of the cæcum, and with the arch of the ileum above the band. It was contracted in ſize, but was nearly free from thickening or inflammation; ſome adheſions only connected it with the coats of the colon; but the portion above the band was at leaſt four times as large, thus reſembling in magnitude as well as occupying the place of the great arch of the colon. Beſides this inteſtine, this cavity contained a portion of the omentum continued from that above, paſſing within the band, and extending half-way to the rectum; an enlarged cluſter of meſenteric glands, of the ſize of a pigeon's egg, which juſt emerged from under the band, and were connected with a portion of the meſentery above; and, at the lower part, the appendix vermiformis larger and longer than natural, but likewiſe uninverted, the mouth of the cavity of which formed the ſmaller opening in the cæcum before mentioned. It was at this point of the diſſection that the ſame ingenious Surgeon drew the figure, tab. VIII.

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* The increaſed action of theſe muſcles, neceſſarily attendant on their inverted ſtate, would increaſe the ſize of their muſcular fibres, as happens in the bladder, when it acts frequently.

As long as the parts had been in this very uncommon ſituation, the fæces muſt have paſſed through the valve of the colon, directly into the very loweſt part of the rectum; without ever coming in contact with any portion of the large inteſtines.

And in the laſt week of the child's life, when a prolapſus frequently happened, the fæces paſſed directly from the ileum into the night-ſtool.

The arch of the ileum, in default of that of the colon, formed the reſervoir for the fæces; which, with the partial interruption to their paſſage by the ſtricture occaſioned by the band, probably cauſed its enlargement. But the fæces contained in it were of a thinner conſiſtence, and wanted the fætor uſually met with in the colon.

EXPLANATION OF THE PLATES.

TAB. VII.

A general view of the inteſtines, in the ſituation in which they appeared on firſt opening the body,

aa. The enlarged ileum, putting on the appearance of the great arch of the colon.

b. The ſudden enlargement of the ileum.

c. The ileum paſſing within the band into the colon.

d. Part of the omentum paſſing within the band.

e. The inteſtinal band, formed by the inverſion of the great arch of the colon immediately above it ceaſing at this place, *ff.* The ſigmoid flexure of the colon, containing the introſuſcepted portion of the alimentary canal.

g. The rectum diſtended with the ſame.

h. The anus.

ii. Small inteſtines of the natural ſize and healthy appearance.

T A B. VIII.

The ſame view, with the ſigmoid flexure laid open, and the edges turned back, to ſhew the contained parts; and likewiſe the introſuſcepted colon laid open, to diſplay the uninverted ileum and appendix vermiformis contained within it.

aaaa. Internal ſurface of the ſigmoid flexure of the colon ſpread open.

bbbb. The external ſurface (by the inverſion now become internal) of the great arch of the colon within the ſigmoid flexure ſpread open.

cc. Part of the ileum uninverted.

d. Appendix cæci uninverted.

ee. A probe piercing the diſtended ileum, paſſed within the band, and brought out in another portion of the ileum, contained within the inverted colon below the band.

ff. A blow-pipe paſſed through the valve of the colon, where it opened into the rectum, and brought out through the coats of the ileum above.

gg. A probe paſſed into the natural opening of the appendix cæci, and brought out above.

hh. The cæcum inverted.

i.

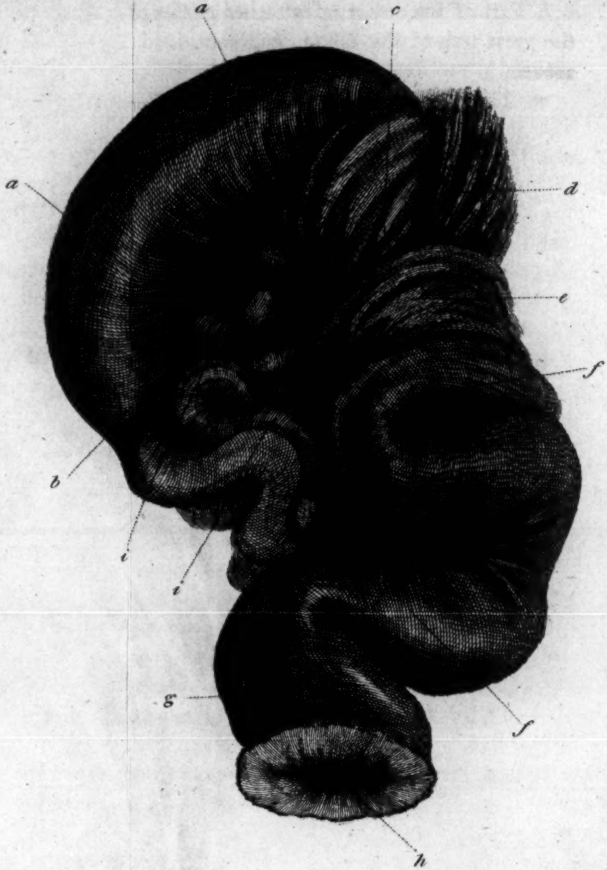
i. Part of the rectum.

k. The anus.

l. Part of the omentum, attached to the peritoneal ſurface of the great arch of the colon, and continued from the portion above.

m. The cluſter of enlarged meſenteric glands.

Philos.Trans.Vol.LXXVI.Tab.VII.*p*.312.



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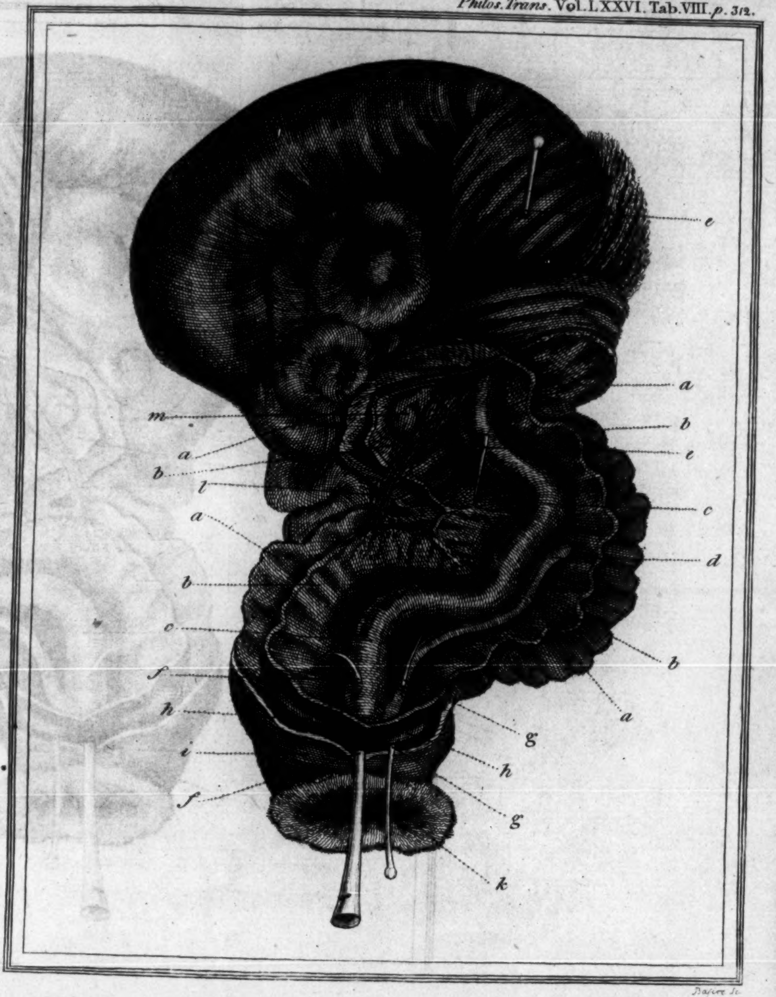

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XVI. *New Experiments on the Ocular Spectra of Light and Colours. By* Robert Waring Darwin, *M. D.; communicated by* Eraſmus Darwin, *M. D. F. R. S.*

Read March 23, 1786.

WHEN any one has long and attentively looked at a bright object, as at the ſetting ſun, on cloſing his eyes, or removing them, an image, which reſembles in form the object he was attending to, continues ſome time to be viſible: this appearance in the eye we ſhall call the ocular ſpectrum of that object.

Theſe ocular ſpectra are of four kinds: 1ſt, Such as are owing to a leſs ſenſibility of a defined part of the retina; or *ſpectra from defect of ſenſibility.* 2d, Such as are owing to a greater ſenſibility of a defined part of the retina; or *ſpectra from exceſs of ſenſibility.* 3d, Such as reſemble their object in its colour as well as form; which may be termed *direct ocular ſpectra.* 4th, Such as are of a colour contrary to that of their object; which may be termed *reverſe ocular ſpectra.*

The laws of light have been moſt ſucceſsfully explained by the great NEWTON, and the perception of viſible objects has been ably inveſtigated by the ingenious Dr. BERKELEY and M. MALEBRANCHE; but theſe minute phænomena of viſion have yet been thought reducible to no theory, though many philoſophers have employed a conſiderable degree of attention upon them: among theſe are Dr. JURIN, at the end of Dr. SMITH's Optics; M.

ÆPINUS, in the Nov. Com. Petropol. V. 10.; M. BEGUELIN, in the Berlin Memoires, V. II. 1771; M. D'ARCY, in the Hiſtoire de l'Acad. des Scienc. 1765; M. DE LA HIRE; and, laſtly, the celebrated M. DE BUFFON, in the Memoires de l'Acad. des Scien. who has termed them accidental colours, as if ſubjected to no eſtabliſhed laws, Ac. Par. 1743. M. p. 215.

I muſt here apprize the reader, that it is very difficult for different people to give the ſame names to various ſhades of colours; whence, in the following pages, ſomething muſt be allowed if, on repeating the experiments, the colours here mentioned ſhould not accurately correſpond with his own names of them.

I. ACTIVITY OF THE RETINA IN VISION.

From the ſubſequent experiments it appears, that the retina is in an active not in a paſſive ſtate during the exiſtence of theſe ocular ſpectra; and it is thence to be concluded, that all viſion is owing to the activity of this organ.

1. Place a piece of red ſilk, about an inch in diameter, as in fig. 1. (Tab. IX.) on a ſheet of white paper, in a ſtrong light; look ſteadily upon it from about the diſtance of half a yard for a minute; then cloſing your eyelids cover them with your hands, and a green ſpectrum will be ſeen in your eyes, reſembling in form the piece of red ſilk: after ſome time, this ſpectrum will diſappear and ſhortly re-appear; and this alternately three or four times, if the experiment is well made, till at length it vaniſhes entirely.

2. Place on a ſheet of white paper a circular piece of blue ſilk, about four inches in diameter, in the ſunſhine; cover the center of this with a circular piece of yellow ſilk, about

three inches in diameter; and the center of the yellow ſilk with a circle of pink ſilk, about two inches in diameter; and the center of the pink ſilk with a circle of green ſilk, about one inch in diameter; and the center of this with a circle of indigo, about half an inch in diameter; make a ſmall ſpeck with ink in the very center of the whole, as in fig. 2.; look ſteadily for a minute on this central ſpot, and then cloſing your eyes, and applying your hand at about an inch diſtance before them, ſo as to prevent too much or too little light from paſſing through the eyelids, you will ſee the moſt beautiful circles of colours that imagination can conceive, which are moſt reſembled by the colours occaſioned by pouring a drop or two of oil on a ſtill lake in a bright day; but theſe circular iriſes of colours are not only different from the colours of the ſilks abovementioned, but are at the ſame time perpetually changing as long as they exiſt.

3. When any one in the dark preſſes either corner of his eye with his finger, and turns his eye away from his finger, he will ſee a circle of colours like thoſe in a peacock's tail: and a ſudden flaſh of light is excited in the eye by a ſtroke on it. (NEWTON's Opt. Qu. 16.)

4. When any one turns round rapidly on one foot, till he becomes dizzy and falls upon the ground, the ſpectra of the ambient objects continue to preſent themſelves in rotation, or appear to librate, and he ſeems to behold them for ſome time ſtill in motion.

From all theſe experiments it appears, that the ſpectra in the eye are not owing to the mechanical impulſe of light impreſſed on the retina, nor to its chemical combination with that organ, nor to the abſorption and emiſſion of light, as is obſerved in many bodies: for in all theſe caſes the ſpectra muſt either remain uniformly, or gradually diminiſh; and neither their alternate preſence and evaneſcence as in the firſt experiment, nor the perpetual changes of their colours as in the ſecond, nor the flaſh of light or colours in the preſſed eye as in the third, nor the rotation or libration of the ſpectra as in the fourth, could exiſt.

It is not abſurd to conceive, that the retina may be ſtimulated into motion, as well as the red and white muſcles which form our limbs and veſſels; ſince it conſiſts of fibres, like thoſe, intermixed with its medullary ſubſtance. To evince this ſtructure, the retina of an ox's eye was ſuſpended in a glaſs of warm water, and forcibly torn in a few places; the edges of theſe parts appeared jagged and hairy, and did not contract, and become ſmooth like ſimple mucus, when it is diſtended till it breaks; which ſhews that it conſiſts of fibres; and this its fibrous conſtruction became ſtill more diſtinct to the ſight, by adding ſome cauſtic alkali to the water, as the adhering mucus was firſt eroded, and the hair-like fibres remained floating in the veſſel. Nor does the degree of tranſparency of the retina invalidate the evidence of its fibrous ſtructure, ſince LEEUWENHOEK has ſhewn that the cryſtalline humour itſelf conſiſts of fibres. (Arcana Naturæ, V. 1. p. 70.)

Hence it appears, that as the muſcles have larger fibres intermixed with a ſmaller quantity of nervous medulla, the organ of viſion has a greater quantity of nervous medulla intermixed with ſmaller fibres; and it is probable, that the locomotive muſcles, as well as the vaſcular ones, of microſcopic animals have much greater tenuity than theſe of the retina.

And beſides the ſimilar laws, which will be ſhewn in this Paper to govern alike the actions of the retina and of the muſcles, there are many other analogies which exiſt between them. They are both originally excited into action by irritations, both act nearly in the ſame quantity of time, are alike ſtrengthened or fatigued by exertion, are alike painful if excited into action when they are in an inflamed ſtate, are alike liable to paralyſis, and to the torpor of old age.

II. OF SPECTRA FROM DEFECT OF SENSIBILITY.

The retina is not ſo eaſily excited into action by leſs irritation after having been lately ſubjected to greater.

1. When any one paſſes from the bright daylight into a darkened room, the iriſes of his eyes expand themſelves to their utmoſt extent in a few ſeconds of time; but it is very long before the optic nerve, after having been ſtimulated by the greater light of the day, becomes ſenſible of the leſs degree of it in the room; and, if the room is not too obſcure, the iriſes will again contract themſelves in ſome degree, as the ſenſibility of the retina returns.

2. Place about half an inch ſquare of white paper on a black hat, and looking ſteadily on the center of it for a minute, remove your eyes to a ſheet of white paper; and after a ſecond or two a dark ſquare will be ſeen on the white paper, which will continue ſome time. A ſimilar dark ſquare will be ſeen in the cloſed eye, if light be admitted through the eye-lids.

So after looking at any luminous object of a ſmall ſize, as at the ſun, for a ſhort time, ſo as not much to fatigue the eyes, this part of the retina becomes leſs ſenſible to ſmaller quantities of light; hence, when the eyes are turned on other leſs luminous parts of the ſky, a dark ſpot is ſeen reſembling the ſhape of the ſun, or other luminous object which we laſt beheld. This is the ſource of one kind of the dark-coloured *muſcæ volitantes*. If this dark ſpot lies above the center of the eye, we turn our eyes that way, expecting to bring it into the center of the eye, that we may view it more diſtinctly; and in this caſe the dark ſpectrum ſeems to move upwards. If the dark ſpectrum is found beneath the center of the eye, we purſue it from the ſame motive, and it ſeems to move downwards. This has given riſe to various conjectures of ſomething floating in the aqueous humours of the eyes; but whoever, in attending to theſe ſpots, keeps his eyes unmoved by looking ſteadily at the corner of a cloud, at the ſame time that he obſerves the dark ſpectra, will be thoroughly convinced, that they have no motion but what is given to them by the movement of our eyes in purſuit of them. Sometimes the form of the ſpectrum, when it has been received from a circular luminous body, will become oblong; and ſometimes it will be divided into two circular ſpectra, which is owing to our changing the angle made by the two optic axiſes, according to the diſtance of the clouds or other bodies to which the ſpectrum is ſuppoſed to be contiguous. The apparent ſize of it will alſo be variable according to its ſuppoſed diſtance; but when ſuch a ſpectrum is received with only one eye, the other being covered, its form and number are invariable.

As theſe ſpectra are more eaſily obſervable when our eyes are a little weakened by fatigue, it has frequently happened, that people of delicate conſtitutions have been much alarmed at them, fearing a beginning decay of their ſight, and have thence fallen into the hands of ignorant oculiſts; but I believe they never are a prelude to any other diſeaſe of the eye, and that it is from habit alone, and our want of attention to them,

that

that we do not ſee them on all objects every hour of our lives. But as the nerves of very weak people loſe their ſenſibility, in the ſame manner as their muſcles loſe their activity, by a ſmall time of exertion, it frequently happens, that ſick people in the extreme debility of fevers are perpetually employed in picking ſomething from the bed-cloaths, occaſioned by their miſtaking the appearance of theſe *muſcæ volitantes* in their eyes. BENVENUTO CELINI, an Italian artiſt, a man of ſtrong abilities, relates, that having paſſed the whole night on a diſtant mountain with ſome companions and a conjurer, and performed many ceremonies to raiſe the devil, on their return in the morning to Rome, and looking up when the ſun began to riſe, they ſaw numerous devils run on the tops of the houſes, as they paſſed along; ſo much were the ſpectra of their weakened eyes magnified by fear, and made ſubſervient to the purpoſes of fraud or ſuperſtition. (Life of BEN. CELINI.)

3. Place a ſquare inch of white paper on a large piece of ſtraw-coloured ſilk; look ſteadily ſome time on the white paper, and then move the center of your eyes on the ſilk, and a ſpectrum of the form of the paper will appear on the ſilk, of a deeper yellow than the other part of it: for the central part of the retina, having been ſome time expoſed to the ſtimulus of a greater quantity of white light, is become leſs ſenſible to a ſmaller quantity of it, and therefore ſees only the yellow rays in that part of the ſtraw-coloured ſilk.

Facts ſimilar to theſe are obſervable in other parts of our ſyſtem: thus, if one hand be made warm, and the other expoſed to the cold, and then both of them immerſed in ſubtepid water, the water is perceived warm to one hand, and cold to the other; and we are not able to hear weak ſounds for ſome time after we have been expoſed to loud ones; and we feel a chillineſs on coming into an atmoſphere of temperate warmth, after having been ſome time confined in a very warm room: and hence the ſtomach, and other organs of digeſtion, of thoſe who have been habituated to the greater ſtimulus of ſpirituous liquor, are not excited into their due action by the leſs ſtimulus of common food alone; of which the immediate conſequence is indigeſtion and hypochondriaciſm.

III. OF SPECTRA FROM EXCESS OF SENSIBILITY.

The retina is more eaſily excited into action by greater irritation after having been lately ſubjected to leſs.

1. If the eyes are cloſed, and covered perfectly with a hat, for a minute or two, in a bright day; on removing the hat a red or crimſon light is ſeen through the eye-lids. In this experiment the retina, after being ſome time kept in the dark, becomes ſo ſenſible to a ſmall quantity of light, as to perceive diſtinctly the greater quantity of red rays than of others which paſs through the eye-lids. A ſimilar coloured light is ſeen to paſs through the edges of the fingers, when the open hand is oppoſed to the flame of a candle.

2. If you look for ſome minutes ſteadily on a window in the beginning of the evening twilight, or in a dark day, and then move your eyes a little, ſo that thoſe parts of the retina, on which the dark frame-work of the window was delineated, may now fall on the glaſs part of it, many luminous lines, repreſenting the frame-work, will appear to lie acroſs the glaſs panes: for thoſe parts of the retina, which were before leaſt ſtimulated by the dark frame-work, are now more ſenſible to

light than the other parts of the retina which were expoſed to the more luminous parts of the window.

3. Make with ink on white paper a very black ſpot, about half an inch in diameter, with a tail about an inch in length, ſo as to repreſent a tadpole; look ſteadily for a minute on this ſpot, and, on moving the eye a little, the figure of the tadpole will be ſeen on the white part of the paper, which figure of the tadpole will appear whiter or more luminous than the other parts of the white paper; for the part of the retina on which the tadpole was delineated, is now more ſenſible to light than the other parts of it, which were expoſed to the white paper. This experiment is mentioned by Dr. IRWIN, but is not by him aſcribed to the true cauſe, namely, the greater ſenſibility of that part of the retina which has been expoſed to the black ſpot, than of the other parts which had received the white field of paper, which is put beyond a doubt by the next experiment.

4. On cloſing the eyes after viewing the black ſpot on the white paper, as in the foregoing experiment, a red ſpot is ſeen of the form of the black ſpot: for that part of the retina, on which the black ſpot was delineated, being now more ſenſible to light than the other parts of it, which were expoſed to the white paper, is capable of perceiving the red rays which penetrate the eyelids. If this experiment be made by the light of a tallow candle, the ſpot will be yellow inſtead of red; for tallow candles abound much with yellow light, which paſſes in greater quantity and force through the eyelids than blue light; hence the difficulty of diſtinguiſhing blue and green by this kind of candle light. The colour of the ſpectrum may poſſibly vary in the day light, according to the different colour of the meridian or the morning or evening light.

M. BEGUELIN, in the Berlin Memoires, V. II. 1771, obſerves, that, when he held a book ſo that the ſun ſhone upon his half-cloſed eyelids, the black letters, which he had long inſpected, became red, which muſt have been thus occaſioned. Thoſe parts of the retina which had received for ſome time the black letters, were ſo much more ſenſible than thoſe parts which had been oppoſed to the white paper, that to the former the red light, which paſſed through the eyelids, was perceptible. There is a ſimilar ſtory told, I think, in M. DE VOLTAIRE's Hiſtorical Works, of a Duke of Tuſcany, who was playing at dice with the general of a foreign army, and, believing he ſaw bloody ſpots upon the dice, portended dreadful events, and retired in confuſion. The obſerver, after looking for a minute on the black ſpots of a die, and careleſſly cloſing his eyes, on a bright day, would ſee the image of a die with red ſpots upon it, as above explained.

5. On emerging from a dark cavern, where we have long continued, the light of a bright day becomes intolerable to the eye for a conſiderable time, owing to the exceſs of ſenſibility exiſting in the eye, after having been long expoſed to little or no ſtimulus. This occaſions us immediately to contract the iris to its ſmalleſt aperture, which becomes again gradually dilated, as the retina becomes accuſtomed to the greater ſtimulus of the daylight.

The twinkling of a bright ſtar, or of a diſtant candle in the night, is perhaps owing to the ſame cauſe. While we continue to look upon theſe luminous objects, their central parts gradually appear paler, owing to the decreaſing ſenſibility of the part of the retina expoſed to their light; whilſt, at the ſame time, by the unſteadineſs of the eye, the edges of them are perpetually falling on parts of the retina that were juſt before expoſed to the darkneſs of the night, and therefore tenfold more ſenſible to light than the part on which the ſtar or candle had been for ſome time delineated. This pains the eye in a ſimilar manner as when we come ſuddenly from a dark room into bright daylight, and gives the appearance of bright ſcintillations. Hence the ſtars twinkle moſt when the night is darkeſt, and do not twinkle through teleſcopes, as obſerved by MUSSCHENBROECK; and it will afterwards be ſeen why this twinkling is ſometimes of different colours when the object is very bright, as Mr. MELVILL obſerved in looking at Sirius. For the opinions of others on this ſubject, ſee Dr. PRIESTLEY'S valuable Hiſtory of Light and Colours, p. 494.

Many facts obſervable in the animal ſyſtem are ſimilar to theſe; as the hot glow occaſioned by the uſual warmth of the air, or our cloaths, on coming out of a cold bath; the pain of the fingers on approaching the fire after having handled ſnow; and the inflamed heels from walking in ſnow. Hence thoſe who have been expoſed to much cold have died on being brought to a fire, or their limbs have become ſo much inflamed as to mortify. Hence much food or wine given ſuddenly to thoſe who have almoſt periſhed by hunger has deſtroyed them; for all the organs of the famiſhed body are now become ſo much more irritable to the ſtimulus of food and wine, which they have long been deprived of, that inflammation is excited, which terminates in gangrene or fever.

IV. OF DIRECT OCULAR SPECTRA.

A quantity of ſtimulus ſomewhat greater than natural excites the retina into ſpaſmodic action, which ceaſes in a few ſeconds.

A certain duration and energy of the ſtimulus of light and colours excites the perfect action of the retina in viſion; for very quick motions are imperceptible to us, as well as very ſlow ones, as the whirling of a top, or the ſhadow on a ſun-dial. So perfect darkneſs does not affect the eye at all; and exceſs of light produces pain, not viſion.

1. When a fire-coal is whirled round in the dark, a lucid circle remains a conſiderable time in the eye; and that with ſo much vivacity of light, that it is miſtaken for a continuance of the irritation of the object. In the ſame manner, when a fiery meteor ſhoots acroſs the night, it appears to leave a long lucid train behind it, part of which, and perhaps ſometimes the whole, is owing to the continuance of the action of the retina after having been thus vividly excited. This is beautifully illuſtrated by the following experiment: fix a paper ſail, three or four inches in diameter, and made like that of a ſmoke jack, in a tube of paſteboard; on looking through the tube at a diſtant proſpect, ſome disjointed parts of it will be ſeen through the narrow intervals between the ſails; but as the fly begins to revolve, theſe intervals appear larger; and when it revolves quicker, the whole proſpect is ſeen quite as diſtinct as if nothing intervened, though leſs luminous.

2. Look through a dark tube, about half a yard long, at the area of a yellow circle of half an inch diameter, lying upon a blue area of double that diameter, for half a minute;

and on cloſing your eyes the colours of the ſpectrum will appear ſimilar to the two areas, as in fig. 3.; but if the eye is kept too long upon them, the colours of the ſpectrum will be the reverſe of thoſe upon the paper, that is, the internal circle will become blue, and the external area yellow; hence ſome attention is required in making this experiment.

3. Place the bright flame of a ſpermaceti candle before a black object in the night; look ſteadily at it for a ſhort time, till it is obſerved to become ſomewhat paler; and on cloſing the eyes, and covering them carefully, but not ſo as to compreſs them, the image of the blazing candle will continue diſtinctly to be viſible.

4. Look ſteadily, for a ſhort time, at a window in a dark day, as in Exp. 2. S. III. and then cloſing your eyes, and covering them with your hands, an exact delineation of the window remains for ſome time viſible in the eye. This experiment requires a little practice to make it ſucceed well; ſince, if the eyes are fatigued by looking too long on the window, or the day be too bright, the luminous parts of the window will appear dark in the ſpectrum, and the dark parts of the frame-work will appear luminous, as in Exp. 2. S. III. And it is even difficult for many, who firſt try this experiment, to perceive the ſpectrum at all; for any hurry of mind, or even too great attention to the ſpectrum itſelf, will diſappoint them, till they have had a little experience in attending to ſuch ſmall ſenſations.

The ſpectra deſcribed in this ſection, termed direct ocular ſpectra, are produced without much fatigue of the eye; the irritation of the luminous object being ſoon withdrawn, or its quantity of light being not ſo great as to produce any degree of uneaſineſs in the organ of viſion; which diſtinguiſhes them from the next claſs of ocular ſpectra, which are the conſequence of fatigue. Theſe direct ſpectra are beſt obſerved in ſuch circumſtances that no light, but what comes from the object, can fall upon the eye; as in looking through a tube, of half a yard long, and an inch wide, at a yellow paper on the ſide of a room, the direct ſpectrum was eaſily produced on cloſing the eye without taking it from the tube: but if the lateral light is admitted through the eye-lids, or by throwing the ſpectrum on white paper, it becomes a reverſe ſpectrum, as will be explained below.

The other ſenſes alſo retain for a time the impreſſions that have been made upon them, or the actions they have been excited into. So if a hard body is preſſed upon the palm of the hand, as is practiſed in tricks of legerdemain, it is not eaſy to diſtinguiſh for a few ſeconds whether it remains or is removed; and taſtes continue long to exiſt vividly in the mouth, as the ſmoke of tobacco, or the taſte of gentian, after the ſapid material is withdrawn.

V. *A quantity of ſtimulus ſomewhat greater than the laſt mentioned excites the retina into ſpaſmodic action, which ceaſes and recurs alternately.*

1. On looking for a time on the ſetting ſun, ſo as not greatly to fatigue the ſight, a yellow ſpectrum is ſeen when the eyes are cloſed and covered, which continues for a time, and then diſappears, and recurs repeatedly before it intirely vaniſhes. This yellow ſpectrum of the ſun when the eyelids are opened becomes blue; and if it is made to fall on the green graſs, or on other coloured objects, it varies its own

colour by an intermixture of theirs, as will be explained in another place.

2. Place a lighted ſpermaceti candle in the night about one foot from your eye, and look ſteadily on the center of the flame, till your eye becomes much more fatigued than in S. IV. Exp. 3.; and on cloſing your eyes a reddiſh ſpectrum will be perceived, which will ceaſe and return alternately.

The action of vomiting in like manner ceaſes, and is renewed by intervals, although the emetic drug is thrown up with the firſt effort: ſo after-pains continue ſome time after parturition; and the alternate pulſations of the heart of a viper are renewed for ſome time after it is cleared from its blood.

VI. OF REVERSE OCULAR SPECTRA.

The retina after having been excited into action by a ſtimulus ſomewhat greater than the laſt mentioned falls into oppoſite ſpaſmodic action.

The actions of every part of animal bodies may be advantageouſly compared with each other. This ſtrict analogy contributes much to the inveſtigation of truth; while thoſe looſer analogies, which compare the phænomena of animal life with thoſe of chemiſtry or mechanics, only ſerve to miſlead our inquiries.

When any of our larger muſcles have been in long or in violent action, and their antagoniſts have been at the ſame time extended, as ſoon as the action of the former ceaſes, the limb is ſtretched the contrary way for our eaſe, and a pandiculation or yawning takes place.

By the following obſervations it appears, that a ſimilar circumſtance obtains in the organ of viſion; after it has been fatigued by one kind of action, it ſpontaneouſly falls into the oppoſite kind.

1. Place a piece of coloured ſilk, about an inch in diameter, on a ſheet of white paper, about half a yard from your eyes; look ſteadily upon it for a minute; then remove your eyes upon another part of the white paper, and a ſpectrum will be ſeen of the form of the ſilk thus inſpected, but of a colour oppoſite to it. A ſpectrum nearly ſimilar will appear if the eyes are cloſed, and the eyelids ſhaded by approaching the hand near them, ſo as to permit ſome but to prevent too much light falling on them.

Red ſilk produced a green ſpectrum.
Green produced a red one.
Orange produced blue.
Blue produced orange.
Yellow produced violet.
Violet produced yellow.

That in theſe experiments the colours of the ſpectra are the reverſe of the colours which occaſioned them, may be ſeen by examining the third figure in Sir Isaac Newton's Optics, L. II. p. 1. where thoſe thin laminæ of air, which reflected yellow, tranſmitted violet; thoſe which reflected red, tranſmitted a blue-green; and ſo of the reſt, agreeing with the experiments above related.

2. Theſe reverſe ſpectra are ſimilar to a colour, formed by a combination of all the primary colours except that with which the eye has been fatigued in making the experiment: thus the reverſe ſpectrum of red muſt be ſuch a green as would be produced by a combination of all the other priſmatic colours.

To evince this fact the following ſatisfactory experiment was made. The priſmatic colours were laid on a circular paſteboard wheel, about four inches in diameter, in the proportions deſcribed in Dr. PRIESTLEY's Hiſtory of Light and Colours, pl. 12. fig. 83. except that the red compartment was intirely left out, and the others proportionably extended ſo as to complete the circle. Then, as the orange is a mixture of red and yellow, and as the violet is a mixture of red and indigo, it became neceſſary to put yellow on the wheel inſtead of orange, and indigo inſtead of violet, that the experiment might more exactly quadrate with the theory it was deſigned to eſtabliſh or confute; becauſe in gaining a green ſpectrum from a red object, the eye is ſuppoſed to have become inſenſible to red light. This wheel, by means of an axis, was made to whirl like a top; and on its being put in motion, a green colour was produced, correſponding with great exactneſs to the reverſe ſpectrum of red.

3. In contemplating any one of theſe reverſe ſpectra in the cloſed and covered eye, it diſappears and re-appears ſeveral times ſucceſſively, till at length it intirely vaniſhes, like the direct ſpectra in ſect. v.; but with this additional circumſtance, that when the ſpectrum becomes faint or evaneſcent, it is inſtantly revived by removing the hand from before the eyelids, ſo as to admit more light: becauſe then not only the fatigued part of the retina is inclined ſpontaneouſly to fall into motions of a contrary direction, but being ſtill ſenſible to all other rays of light, except that with which it was lately fatigued, is by theſe rays at the ſame time ſtimulated into thoſe motions which form the reverſe ſpectrum.

From theſe experiments there is reaſon to conclude, that the fatigued part of the retina throws itſelf into a contrary mode of action, like oſcitation or pandiculation, as ſoon as the ſtimulus which has fatigued it is withdrawn; and that it ſtill remains ſenſible, that is, liable to be excited into action by any other colours at the ſame time, except the colour with which it has been fatigued.

VII. *The retina after having been excited into action by a ſtimulus ſomewhat greater than the laſt mentioned falls into various ſucceſſive ſpaſmodic actions.*

1. On looking at the meridian ſun as long as the eyes can well bear its brightneſs, the diſc firſt becomes pale, with a luminous creſcent, which ſeems to librate from one edge of it to the other, owing to the unſteadineſs of the eye; then the whole phaſis of the ſun becomes blue, ſurrounded with a white halo; and on cloſing the eyes, and covering them with the hands, a yellow ſpectrum is ſeen, which in a little time changes into a blue one.

M. DE LA HIRE obſerved, after looking at the bright ſun, that the impreſſion in his eye firſt aſſumed a yellow appearance, and then green, and then blue; and wiſhes to aſcribe theſe appearances to ſome affection of the nerves. (PORTERFIELD on the Eye, Vol. I. p. 343.)

2. After looking ſteadily on about an inch ſquare of pink ſilk, placed on white paper, in a bright ſunſhine, at the diſtance of a foot from my eyes, and cloſing and covering my eyelids, the ſpectrum of the ſilk was at firſt a dark green, and the ſpectrum of the white paper became of a pink. The ſpectra then both diſappeared; and then the internal ſpectrum was blue; and then, after a ſecond diſappearance, became yellow,

and laſtly pink, whilſt the ſpectrum of the field varied into red and green.

Theſe ſucceſſions of different coloured ſpectra were not exactly the ſame in the different experiments, though obſerved, as near as could be, with the ſame quantity of light, and other ſimilar circumſtances; owing, I ſuppoſe, to trying too many experiments at a time; ſo that the eye was not quite free from the ſpectra of the colours which were previouſly attended to.

The alternate exertions of the retina in the preceding ſection reſembled the oſcitation or pandiculation of the muſcles, as they were performed in directions contrary to each other, and were the conſequence of fatigue rather than of pain. And in this they differ from the ſucceſſive diſſimilar exertions of the retina, mentioned in this ſection, which reſemble in miniature the more violent agitations of the limbs in convulſive diſeaſes, as epilepſy, chorea S. Viti, and opiſthotonos; all which diſeaſes are perhaps, at firſt, the conſequence of pain, and have their periods afterwards eſtabliſhed by habit.

VIII. *The retina, after having been excited into action by a ſtimulus ſomewhat greater than the laſt mentioned, falls into a fixed ſpaſmodic action, which continues for ſome days.*

1. After having looked long at the meridian ſun, in making ſome of the preceding experiments, till the diſcs faded into a pale blue, I frequently obſerved a bright blue ſpectrum of the ſun on other objects all the next and the ſucceeding day, which conſtantly occurred when I attended to it, and frequently when I did not previouſly attend to it. When I cloſed and covered

my eyes, this appeared of a dull yellow; and at other times mixed with the colours of other objects on which it was thrown. It may be imagined, that this part of the retina was become inſenſible to white light, and thence a bluiſh ſpectrum became viſible on all luminous objects; but as a yellowiſh ſpectrum was alſo ſeen in the cloſed and covered eye, there can remain no doubt of this being the ſpectrum of the ſun. A ſimilar appearance was obſerved by M. ÆPINUS, which he acknowledges he could give no account of. (Nov. Com. Petrop. V. 10, p. 2. and 6.)

The locked jaw, and ſome cataleptic ſpaſms, are reſembled by this phænomenon; and from hence we may learn the danger to the eye by inſpecting very luminous objects too long a time.

IX. *A quantity of ſtimulus greater than the preceding induces a temporary paralyſis of the organ of viſion.*

1. Place a circular piece of bright red ſilk, about half an inch in diameter, on the middle of a ſheet of white paper; lay them on the floor in a bright ſunſhine, and fixing your eyes ſteadily on the center of the red circle, for three or four minutes, at the diſtance of four or ſix feet from the object, the red ſilk will gradually become paler, and finally ceaſe to appear red at all.

2. Similar to theſe are many other animal facts; as purges, opiates, and even poiſons, and contagious matter, ceaſe to ſtimulate our ſyſtem, after we have been habituated to their uſe. So ſome people ſleep undiſturbed by a clock, or even by a forge hammer in their neighbourhood: and not only continued irritations, but violent exertions of any kind, are ſucceeded

by temporary paralyſis. The arm drops down after violent action, and continues for a time uſeleſs; and it is probable, that thoſe who have periſhed ſuddenly in ſwimming, or in ſcating on the ice, have owed their deaths to the paralyſis, or extreme fatigue, which ſucceeds every violent and continued exertion.

X. MISCELLANEOUS REMARKS.

There were ſome circumſtances occurred in making theſe experiments, which were liable to alter the reſults of them, and which I ſhall here mention for the aſſiſtance of others, who may wiſh to repeat them.

1. *Of direct and inverſe ſpectra exiſting at the ſame time; of reciprocal direct ſpectra; of a combination of direct and inverſe ſpectra; of a ſpectral halo; rules to pre-determine the colours of ſpectra.*

a. When an area, about ſix inches ſquare, of bright pink Indian paper, had been viewed on an area, about a foot ſquare, of white writing paper, the internal ſpectrum in the cloſed eye was green, being the reverſe ſpectrum of the pink paper; and the external ſpectrum was pink, being the direct ſpectrum of the pink paper. The ſame circumſtance happened when the internal area was white, and external one pink; that is, the internal ſpectrum was pink, and the external one green. All the ſame appearances occurred when the pink paper was laid on a black hat.

b. When ſix inches ſquare of deep violet poliſhed paper was viewed on a foot ſquare of white writing paper, the internal ſpectrum was yellow, being the reverſe ſpectrum of the violet paper, and the external one was violet, being the direct ſpectrum of the violet paper.

c. When ſix inches ſquare of pink paper was viewed on a foot ſquare of blue paper, the internal ſpectrum was blue, and the external ſpectrum was pink; that is, the internal one was the direct ſpectrum of the external object, and the external one was the direct ſpectrum of the internal object, inſtead of their being each the reverſe ſpectrum of the objects they belonged to.

d. When ſix inches ſquare of blue paper were viewed on a foot ſquare of yellow paper, the interior ſpectrum became a brilliant yellow, and the exterior one a brilliant blue. The vivacity of the ſpectra was owing to their being excited both by the ſtimulus of the interior and exterior objects; ſo that the interior yellow ſpectrum was both the reverſe ſpectrum of the blue paper, and the direct one of the yellow paper; and the exterior blue ſpectrum was both the reverſe ſpectrum of the yellow paper, and the direct one of the blue paper.

e. When the internal area was only a ſquare half-inch of red paper, laid on a ſquare foot of dark violet paper, the internal ſpectrum was green, with a reddiſh-blue halo. When the red internal paper was two inches ſquare, the internal ſpectrum was a deeper green, and the external one redder. When the internal paper was ſix inches ſquare, the ſpectrum of it became blue, and the ſpectrum of the external paper was red.

f. When a ſquare half-inch of blue paper was laid on a ſix-inch ſquare of yellow paper, the ſpectrum of the central paper in the cloſed eye was yellow, incircled with a blue halo. On looking long on the meridian ſun, the diſc fades into a pale blue ſurrounded with a whitiſh halo.

These circumſtances, though they very much perplexed the experiments till they were inveſtigated, admit of a ſatisfactory explanation; for while the rays from the bright internal object in exp. *a.* fall with their full force on the center of the retina, and, by fatiguing that part of it, induce the reverſe ſpectrum, many ſcattered rays, from the ſame internal pink paper, fall on the more external parts of the retina, but not in ſuch quantity as to occaſion much fatigue, and hence induce the direct ſpectrum of the pink colour in thoſe parts of the eye. The ſame reverſe and direct ſpectra occur from the violet paper in exp. *b.*: and in exp. *c.* the ſcattered rays from the central pink paper produce a direct ſpectrum of this colour on the external parts of the eye, while the ſcattered rays from the external blue paper produce a direct ſpectrum of that colour on the central part of the eye, inſtead of theſe parts of the retina falling reciprocally into their reverſe ſpectra. In exp. *d.* the colours being the reverſe of each other, the ſcattered rays from the exterior object falling on the central parts of the eye, and there exciting their direct ſpectrum, at the ſame time that the retina was excited into a reverſe ſpectrum by the central object, and this direct and reverſe ſpectrum being of ſimilar colour, the ſuperior brilliancy of this ſpectrum was produced. In exp. *e.* the effect of various quantities of ſtimulus on the retina, from the different reſpective ſizes of the internal and external areas, induced a ſpectrum of the internal area in the center of the eye, combined of the reverſe ſpectrum of that internal area and the direct one of the external area, in various ſhades of colour, from a pale green to a deep blue, with ſimilar changes in the ſpectrum of the external area. For the ſame reaſons, when an internal bright object was ſmall, as in exp. *f.* inſtead of the whole of the ſpectrum of the external object being reverſe to the colour of the internal object, only a kind of halo, or radiation of colour, ſimilar to that of the internal object, was ſpread a little way on the external ſpectrum. For this internal blue area being ſo ſmall, the ſcattered rays from it extended but a little way on the image of the external area of yellow paper, and could therefore produce only a blue halo round the yellow ſpectrum in the center.

If any one ſhould ſuſpect that the ſcattered rays from the exterior coloured object do not intermix with the rays from the interior coloured object, and thus affect the central part of the eye, let him look through an opake tube, about two feet in length, and an inch in diameter, at a coloured wall of a room with one eye, and with the other eye naked; and he will find, that by ſhutting out the lateral light, the area of the wall ſeen through a tube appears as if illuminated by the ſunſhine, compared with the other parts of it; from whence ariſes the advantage of looking through a dark tube at diſtant paintings.

Hence we may ſafely deduce the following rules to determine before-hand the colours of all ſpectra. 1. *The direct ſpectrum* without any lateral light is an evaneſcent repreſentation of its object in the unfatigued eye. 2. With ſome lateral light it becomes of a colour combined of the direct ſpectrum of the central object, and of the circumjacent objects, in proportion to their reſpective quantity and brilliancy. 3. The *reverſe ſpectrum* without lateral light is a repreſentation in the fatigued eye of the form of its objects, with ſuch a colour as would be produced by all the primary colours, except that of the object. 4. With lateral light the colour is compounded of the reverſe ſpectrum of the central object, and the direct ſpectrum of the circumjacent objects, in proportion to their reſpective quantity and brilliancy.

II. *Variation and vivacity of the ſpectra occaſioned by extraneous light.*

The reverſe ſpectrum, as has been before explained, is ſimilar to a colour, formed by a combination of all the primary colours, except that with which the eye has been fatigued in making the experiment: ſo the reverſe ſpectrum of red is ſuch a green as would be produced by a combination of all the other priſmatic colours. Now it muſt be obſerved, that this reverſe ſpectrum of red is therefore the direct ſpectrum of a combination of all the other priſmatic colours, except the red; whence, on removing the eye from a piece of red ſilk to a ſheet of white paper, the green ſpectrum, which is perceived, may either be called the reverſe ſpectrum of the red ſilk, or the direct ſpectrum of all the rays from the white paper, except the red; for in truth it is both. Hence we ſee the reaſon why it is not eaſy to gain a direct ſpectrum of any coloured object in the day-time, where there is much lateral light, except of very bright objects, as of the ſetting ſun, or by looking through an opake tube; becauſe the lateral external light falling alſo on the central part of the retina, contributes to induce the reverſe ſpectrum, which is at the ſame time the direct ſpectrum of that lateral light, deducting only the colour of the central object which we have been viewing. And for the ſame reaſon, it is difficult to gain the reverſe ſpectrum, where there is no lateral light to contribute to its formation. Thus, in looking through an opake tube on a yellow wall, and cloſing my eye, without admitting any lateral light, the ſpectra were all at firſt yellow; but at length changed into blue. And on looking in the ſame manner on red paper, I did at length get a green ſpectrum; but they were all at firſt red ones: and the ſame after looking at a candle in the night.

The reverſe ſpectrum was formed with greater facility when the eye was thrown from the object on a ſheet of white paper, or when light was admitted through the cloſed eyelids; becauſe not only the fatigued part of the retina was inclined ſpontaneouſly to fall into motions of a contrary direction; but being ſtill ſenſible to all other rays of light except that with which it was lately fatigued, was by theſe rays ſtimulated at the ſame time into thoſe motions which form the reverſe ſpectrum. Hence, when the reverſe ſpectrum of any colour became faint, it was wonderfully revived by admitting more light through the eyelids, by removing the hand from before them: and hence, on covering the cloſed eyelids, the ſpectrum would often ceaſe for a time, till the retina became ſenſible to the ſtimulus of the ſmaller quantity of light, and then it recurred. Nor was the ſpectrum only changed in vivacity, or in degree, by this admiſſion of light through the eyelids; but it frequently happened, after having viewed bright objects, that the ſpectrum in the cloſed and covered eye was changed into a third ſpectrum, when light was admitted through the eyelids: which third ſpectrum was compoſed of ſuch colours as could paſs through the eyelids, except thoſe of the object. Thus, when an area of half an inch diameter of pink paper was viewed on a ſheet of white paper in the ſunſhine, the ſpectrum with cloſed and covered eyes was green; but on removing the hands from before the cloſed eyelids, the ſpectrum became yellow, and returned inſtantly again to green, as often as the hands were applied to cover the eyelids, or removed from them: for the retina being now inſenſible to red light, the yellow rays paſſing through the eyelids in greater quantity than the other colours, induced a yellow ſpectrum; whereas if the ſpectrum was thrown on white paper, with the eyes open, it became only a lighter green.

Though a certain quantity of light facilitates the formation of the reverſe ſpectrum, a greater quantity prevents its formation, as the more powerful ſtimulus excites even the fatigued parts of the eye into action; otherwiſe we ſhould ſee the ſpectrum of the laſt viewed object as often as we turn our eyes. Hence the reverſe ſpectra are beſt ſeen by gradually approaching the hand near the cloſed eyelids to a certain diſtance only, which muſt be varied with the brightneſs of the day, or the energy of the ſpectrum. Add to this, that all dark ſpectra, as black, blue, or green, if light be admitted through the eyelids, after they have been ſome time covered, give reddiſh ſpectra, for the reaſons given in ſect. III. exp. 1.

From theſe circumſtances of the extraneous light coinciding with the ſpontaneous efforts of the fatigued retina to produce a reverſe ſpectrum, as was obſerved before, it is not eaſy to gain a direct ſpectrum, except of objects brighter than the ambient light; ſuch as a candle in the night, the ſetting ſun, or viewing a bright object through an opake tube; and then the reverſe ſpectrum is inſtantaneouſly produced by the admiſſion of ſome external light; and is as inſtantly converted again to the direct ſpectrum by the excluſion of it. Thus, on looking at the ſetting ſun, on cloſing the eyes, and covering them, a yellow ſpectrum is ſeen, which is the direct ſpectrum of the ſetting ſun; but on opening the eyes on the ſky, the yellow ſpectrum is immediately changed into a blue one, which is the reverſe ſpectrum of the yellow ſun, or the direct ſpectrum of the blue ſky, or a combination of both. And this is again transformed into a yellow one on cloſing the eyes, and ſo reciprocally, as quick as the motions of the opening and cloſing eyelids. Hence, when Mr. MELVILL obſerved the ſcintillations of the ſtar Sirius to be ſometimes coloured, theſe were probably the direct ſpectrum of the blue ſky on the parts of the retina fatigued by the white light of the ſtar! (Eſſays Phyſical and Literary, p. 81. V. 2.)

When a direct ſpectrum is thrown on colours darker than itſelf, it mixes with them; as the yellow ſpectrum of the ſetting ſun, thrown on the green graſs, becomes a greener yellow. But when a direct ſpectrum is thrown on colours brighter than itſelf, it becomes inſtantly changed into the reverſe ſpectrum, which mixes with thoſe brighter colours. So the yellow ſpectrum of the ſetting ſun thrown on the luminous ſky becomes blue, and changes with the colour or brightneſs of the clouds on which it appears. But the reverſe ſpectrum mixes with every kind of colour on which it is thrown, whether brighter than itſelf or not: thus the reverſe ſpectrum, obtained by viewing a piece of yellow ſilk, when thrown on white paper was a lucid blue green; when thrown on black Turkey leather becomes a deep violet. And the ſpectrum of blue ſilk, thrown on white paper, was a light yellow; on black ſilk was an obſcure orange; and the blue ſpectrum, obtained from orange-coloured ſilk, thrown on yellow, became a green.

In theſe caſes the retina is thrown into activity or ſenſation by the ſtimulus of external colours, at the ſame time that it continues the activity or ſenſation which forms the ſpectra; in the ſame manner as the priſmatic colours, painted on a whirling top, are ſeen to mix together. When theſe colours of external objects are brighter than the direct ſpectrum which is thrown upon them, they change it into the reverſe ſpectrum, like the admiſſion of external light on a direct ſpectrum, as explained above. When they are darker than the direct ſpectrum,

trum, they mix with it, their weaker ſtimulus being inſufficient to induce the reverſe ſpectrum.

III. *Variation of ſpectra in reſpect to number and figure and remiſſion.*

When we look long and attentively at any object, the eye cannot always be kept intirely motionleſs; hence, on inſpecting a circular area of red ſilk placed on white paper, a lucid creſcent or edge is ſeen to librate on one ſide or other of the red circle: for the exterior parts of the retina ſometimes falling on the edge of the central ſilk, and ſometimes on the white paper, are leſs fatigued with red light than the central part of the retina, which is conſtantly expoſed to it; and therefore, when they fall on the edge of the red ſilk, they perceive it more vividly. Afterwards, when the eye becomes fatigued, a green ſpectrum in the form of a creſcent is ſeen to librate on one ſide or other of the central circle, as by the unſteadineſs of the eye a part of the fatigued retina falls on the white paper; and as by the increaſing fatigue of the eye the central part of the ſilk appears paler, the edge on which the unfatigued part of the retina occaſionally falls will appear of a deeper red than the original ſilk, becauſe it is compared with the pale internal part of it. M. DE BUFFON in making this experiment obſerved, that the red edge of the ſilk was not only deeper coloured than the original ſilk; but, on his retreating a little from it, it became oblong, and at length divided into two, which muſt have been owing to a change of the angle of the two optic axiſes with the new diſtance he obſerved it at. Thus, if a pen is held up before a diſtant candle, when we look intenſely at the pen two candles are ſeen behind it; when we look intenſely at the candle two pens are ſeen. If the ſight be unſteady at the time of beholding the ſun, even though one eye only be uſed, many images of the ſun will appear, or luminous lines, when the eye is cloſed. And as ſome parts of theſe will be more vivid than others, and ſome parts of them will be produced nearer the center of the eye than others, theſe will diſappear ſooner than the others; and hence the number and ſhape of theſe ſpectra of the ſun will continually vary, as long as they exiſt. The cauſe of ſome being more vivid than others, is the unſteadineſs of the eye of the beholder, ſo that ſome parts of the retina have been longer expoſed to the ſunbeams. That ſome parts of a complicated ſpectrum fade and return before other parts of it, the following experiment evinces. Draw three concentric circles; the external one an inch and a half in diameter, the middle one an inch, and the internal one half an inch; colour the external and internal areas blue, and the remaining one yellow, as in fig. 4.; after having looked about a minute on the center of theſe circles, in a bright light, the ſpectrum of the external area appears firſt in the cloſed eye, then the middle area, and laſtly the central one; and then the central one diſappears, and the others in inverted order. If concentric circles of more colours are added, it produces the beautiful ever changing ſpectrum in ſect. I. exp. 2.

From hence it would ſeem, that the center of the eye produces quicker remiſſions of ſpectra, owing perhaps to its greater ſenſibility; that is, to its more energetic exertions. Theſe remiſſions of ſpectra bear ſome analogy to the tremors of the hands, and palpitations of the heart, of weak people: and perhaps a criterion of the ſtrength of any muſcle or nerve may be taken from the time it can be continued in exertion.

IV. *Variation of ſpectra in reſpect to brilliancy; the viſibility of the circulation of the blood in the eye.*

1. The meridian or evening light makes a difference in the colours of ſome ſpectra; for as the ſun deſcends, the red rays, which are leſs refrangible by the convex atmoſphere, abound in great quantity. Whence the ſpectrum of the light parts of a window at this time, or early in the morning, is red; and becomes blue either a little later or earlier; and white in the meridian day; and is alſo variable from the colour of the clouds or ſky which are oppoſed to the window.

2. All theſe experiments are liable to be confounded, if they are made too ſoon after each other, as the remaining ſpectrum will mix with the new ones. This is a very troubleſome circumſtance to painters, who are obliged to look long upon the ſame colour; and in particular to thoſe whoſe eyes, from natural debility, cannot long continue the ſame kind of exertion. For the ſame reaſon, in making theſe experiments, the reſult becomes much varied if the eyes, after viewing any object, are removed on other objects for but an inſtant of time, before we cloſe them to view the ſpectrum; for the light from the object, of which we had only a tranſient view, in the very time of cloſing our eyes acts as a ſtimulus on the fatigued retina; and for a time prevents the deſired ſpectrum from appearing, or mixes its own ſpectrum with it. Whence, after the eyelids are cloſed, either a dark field, or ſome unexpected colours, are beheld for a few ſeconds, before the deſired ſpectrum becomes diſtinctly viſible.

3. The length of time taken up in viewing an object, of which we are to obſerve the ſpectrum, makes a great difference

in the appearance of the ſpectrum, not only in its vivacity, but in its colour; as the direct ſpectrum of the central object, or of the circumjacent ones, and alſo the reverſe ſpectra of both, with their various combinations, as well as the time of their duration in the eye, and of their remiſſions or alternations, depend upon the degree of fatigue the retina is ſubjected to. The Chevalier D'ARCY conſtructed a machine by which a coal of fire was whirled round in the dark, and found, that when a luminous body made a revolution in eight thirds of time, it preſented to the eye a complete circle of fire; from whence he concludes, that the impreſſion continues on the organ about the ſeventh part of a ſecond. (Mém. de l'Acad. des Sc. 1765.) This, however, is only to be conſidered as the ſhorteſt time of the duration of theſe direct ſpectra; ſince in the fatigued eye both the direct and reverſe ſpectra, with their intermiſſions, appear to take up many ſeconds of time, and ſeem very variable in proportion to the circumſtances of fatigue or energy.

4. It ſometimes happens, if the eyeballs have been rubbed hard with the fingers, that lucid ſparks are ſeen in quick motion amidſt the ſpectrum we are attending to. This is ſimilar to the flaſhes of fire from a ſtroke on the eye in fighting, and is reſembled by the warmth and glow which appear upon the ſkin after friction, and is probably owing to an acceleration of the arterial blood into the veſſels emptied by the previous preſſure. By being accuſtomed to obſerve ſuch ſmall ſenſations in the eye, it is eaſy to ſee the circulation of the blood in this organ. I have attended to this frequently, when I have obſerved my eyes more than commonly ſenſible to other ſpectra. The circulation may be ſeen either in both eyes at a time, or only in one of them; for as a certain quantity of light is neceſſary

to produce this curious phænomenon, if one hand be brought nearer the cloſed eyelids than the other, the circulation in that eye will for a time diſappear. For the eaſier viewing the circulation, it is ſometimes neceſſary to rub the eyes with a certain degree of force after they are cloſed, and to hold the breath rather longer than is agreeable, which, by accumulating more blood in the eye, facilitates the experiment; but in general it may be ſeen diſtinctly after having examined other ſpectra with your back to the light, till the eyes become weary; then having covered your cloſed eyelids for half a minute, till the ſpectrum is faded away which you were examining, turn your face to the light, and removing your hands from the eyelids, by and by again ſhade them a little, and the circulation becomes curiouſly diſtinct. The ſtreams of blood are however generally ſeen to unite, which ſhews it to be the venous circulation, owing, I ſuppoſe, to the greater opacity of the colour of the blood in theſe veſſels; for this venous circulation is alſo much more eaſily ſeen by the microſcope in the tail of a tadpole.

v. *Variation of ſpectra in reſpect to diſtinctneſs and ſize; with a new way of magnifying objects.*

1. It was before obſerved, that when the two colours viewed together were oppoſite to each other, as yellow and blue, red and green, &c. according to the table of reflections and tranſmiſſions of light in Sir ISAAC NEWTON's Optics, B. II. fig. 3. the ſpectra of thoſe colours were of all others the moſt brilliant, and beſt defined; becauſe they were combined of the reverſe ſpectrum of one colour, and of the direct ſpectrum of the other. Hence, in books printed with ſmall types, or in

the minute graduation of thermometers, or of clock-faces, which are to be ſeen at a diſtance, if the letters or figures are coloured with orange, and the ground with indigo; or the letters with red, and the ground with green; or any other lucid colour is uſed for the letters, the ſpectrum of which is ſimilar to the colour of the ground; ſuch letters will be ſeen much more diſtinctly, and with leſs confuſion, than in black or white: for as the ſpectrum of the letter is the ſame colour with the ground on which they are ſeen, the unſteadineſs of the eye in long attending to them will not produce coloured lines by the edges of the letters, which is the principal cauſe of their confuſion. The beauty of colours lying in vicinity to each other, whoſe ſpectra are thus reciprocally ſimilar to each colour, is owing to this greater eaſe that the eye experiences in beholding them diſtinctly; and it is probable, in the organ of hearing a ſimilar circumſtance may conſtitute the pleaſure of melody. Sir ISAAC NEWTON obſerves, that gold and indigo were agreeable when viewed together; and thinks there may be ſome analogy between the ſenſations of light and ſound. (Optics, Qu. 14.)

In viewing the ſpectra of bright objects, as of an area of red ſilk of half an inch diameter on white paper, it is eaſy to magnify it to tenfold its ſize: for if, when the ſpectrum is formed, you ſtill keep your eye fixed on the ſilk area, and remove it a few inches further from you, a green circle is ſeen round the red ſilk: for the angle now ſubtended by the ſilk is leſs than it was when the ſpectrum was formed, but that of the ſpectrum continues the ſame, and our imagination places them at the ſame diſtance. Thus when you view a ſpectrum on a ſheet of white paper, if you approach the paper to the eye, you may diminiſh it to a point; and if the paper is made to recede from the eye, the ſpectrum will appear magnified in proportion to the diſtance.

I was ſurpriſed, and agreeably amuſed, with the following experiment. I covered a paper about four inches ſquare with yellow, and with a pen filled with a blue colour wrote upon the middle of it the word BANKS in capitals, as in fig. 5. and, ſitting with my back to the ſun, fixed my eyes for a minute exactly on the center of the letter N in the middle of the word; after cloſing my eyes, and ſhading them ſomewhat with my hand, the word was diſtinctly ſeen in the ſpectrum in yellow letters on a blue field; and then, on opening my eyes on a yellowiſh wall at twenty feet diſtance, the magnified name of BANKS appeared written on the wall in golden characters.

CONCLUSION.

It was obſerved by the learned M. SAUVAGES (Noſol. method. Cl. VIII. Ord. 1.) that the pulſations of the optic artery might be perceived by looking attentively on a white wall well illuminated. A kind of net-work, darker than the other parts of the wall, appears and vaniſhes alternately with every pulſation. This change of the colour of the wall he well aſcribes to the compreſſion of the retina by the diaſtole of the artery. The various colours produced in the eye by the preſſure of the finger, or by a ſtroke on it, as mentioned by Sir ISAAC NEWTON, ſeem likewiſe to originate from the unequal preſſure on various parts of the retina. Now as Sir ISAAC NEWTON has ſhewn, that all the different colours are reflected or tranſmitted by the laminæ of ſoap bubbles, or of air, according to their different thickneſs or thinneſs, is it not probable, that the effect of the activity of the retina may be to alter its thickneſs or thinneſs, ſo as better to adapt it to reflect or tranſmit the colours which ſtimulate it into action? May not muſcular fibres exiſt in the retina for this purpoſe, which may be leſs minute than the locomotive muſcles of microſcopic animals? May not theſe muſcular actions of the retina conſtitute the ſenſation of lights and colours; and the voluntary repetitions of them, when the object is withdrawn, conſtitute our memory of them? And laſtly, may not the laws of the ſenſations of light, here inveſtigated, be applicable to all our other ſenſes, and much contribute to elucidate many phænomena of animal bodies both in their healthy and diſeaſed ſtate; and thus render this inveſtigation well worthy the attention of the phyſician, the metaphyſician, and the natural philoſopher?

Derby, November 1, 1785.

Fig. 1.

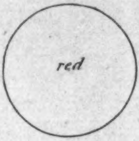


Fig. 2.

Fig. 4.

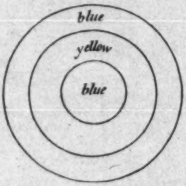
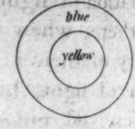


Fig. 3.


blue
yellow

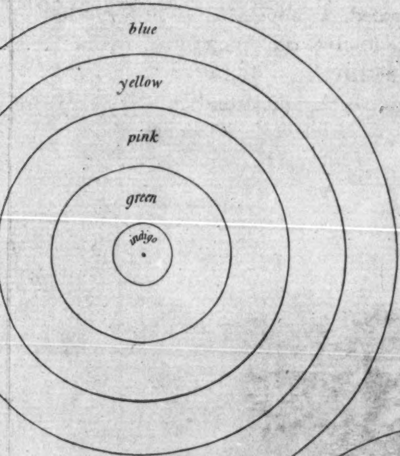
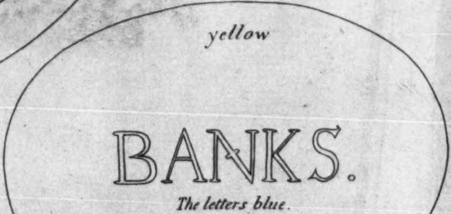

blue
yellow
pink
green
indigo

Fig. 5.


yellow
BANKS.
The letters blue.

VII. *Obſervations on ſome Cauſes of the Exceſs of the Mortality of Males above that of Females.* By Joſeph Clarke, *M. D. Phyſician to the Lying-in Hoſpital at Dublin. Communicated by the Rev.* Richard Price, *D. D. F. R. S. in a Letter to* Charles Blagden, *M. D. Sec. R. S.*

Read March 30, 1786.

SIR, Newington-Green, March 8, 1786.

I BEG leave to ſend you the incloſed papers and tables, drawn up by Dr. Clarke, Phyſician to the Lying-in Hoſpital at Dublin. They contain ſome accounts that ſeem to me fit to be laid before the Royal Society.

The obſervations which have been made on the laws that govern human mortality prove, that the mortality of males exceeds that of females in almoſt all the ſtages of life, and particularly in the earlieſt ſtages; and that this exceſs prevails moſt in great towns, and all the leſs natural ſituations of human life. The facts in theſe papers throw ſome light on this ſubject. Male *fœtus's* requiring more nutrition than female *fœtus's*, becauſe larger, and being alſo for this reaſon more liable to injury in delivery, are brought into the world leſs perfect; and this happening more or leſs in proportion to the vigour and *juſt* formation of the mother, it muſt happen moſt in thoſe ſituations where the greateſt tendencies of diſeaſe and deviations from nature take place. The truth, in

ſhort,

XVII. *Obſervations on ſome Cauſes of the Exceſs of the Mortality of Males above that of Females. By* Joſeph Clarke, *M. D. Phyſician to the Lying-in Hoſpital at* Dublin. *Communicated by the Rev.* Richard Price, *D. D. F. R. S. in a Letter to* Charles Blagden, *M. D. Sec. R. S.*

Read March 30, 1786.

SIR, Newington-Green, February 6, 1786.

I RECEIVED ſome time ago the incloſed letters and regiſtry from Dr. CLARKE, Phyſician to the Lying-in Hoſpital at Dublin. They contain ſome accounts that ſeem to me not improper to be communicated to the Royal Society.

The obſervations which have been made on the laws that govern human mortality prove, that the mortality of males exceeds that of females in almoſt all the ſtages of life, and particularly in the earlieſt ſtages; and that this exceſs prevails moſt in great towns, and all the leſs natural ſituations of human life. The facts in theſe papers throw ſome light on this ſubject. Male *fœtus's* requiring more nutrition than female *fœtus's*, becauſe larger, and being alſo for this reaſon more liable to injury in delivery, are brought into the world leſs perfect: and this happening more or leſs in proportion to the vigour and juſt formation of the mother, it muſt happen moſt in thoſe ſituations where the greateſt tenderneſs of frame and deviations from nature take place. The truth, in ſhort, ſeems to be, that any debility in *either* parent muſt affect moſt the production of that ſex which requires the largeſt and ſtrongeſt *ſtamina*; and that ſuch debilities prevailing moſt in great towns and poliſhed Societies, the exceſs of the mortality of males muſt alſo be greateſt in ſuch ſituations. And this I reckon the principal reaſon of a circumſtance in human mortality which, before I received theſe communications from Dr. CLARKE, I did not ſo well underſtand.

With much reſpect I am, &c.

RICH. PRICE.

Dr. CLARKE's *firſt Letter to the Rev. Dr.* PRICE.

SIR, Dublin.

IN your very uſeful Treatiſe on Life Annuities, &c. you remark[*], that "it has been obſerved, that the Author of "nature has provided, that more *males* ſhould be born than "females, on account of the particular waſte of males, occa"ſioned by wars and other cauſes. That perhaps it might "have been obſerved, with more reaſon, that this proviſion "had in view that particular weakneſs or delicacy in the con"ſtitution of males which makes them more ſubject to mor"tality; and which, conſequently, renders it neceſſary that "more of them ſhould be produced, in order to preſerve

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[*] Vol. I. p. 373.

"in the world a due proportion between the ſexes." And further, you elſewhere remark*, that "the *facts* recited at "the end of your fourth Eſſay *prove*, that there is a difference "between the mortality of males and females; but that you "muſt however obſerve, that it may be *doubted*, whether this "difference, ſo unfavourable to males, be *natural*; and that "there are facts which prove that you have reaſon for ſuch a "doubt." After ſtating a number of very ſatisfactory facts of this kind you remark, that "the inference from them is very "obvious; that they ſeem to ſhew ſufficiently, that human "life in males is more brittle than in females, only in conſe- "quence of adventitious cauſes, or of ſome particular debility "which takes place in poliſhed and luxurious ſocieties, and "eſpecially in great *towns*."

What thoſe adventitious cauſes are, or how this particular debility is produced and operates, are queſtions which appear to me highly intereſting and curious. I have therefore been at conſiderable pains to examine and arrange a very accurate and extenſive regiſtry in ſuch a manner as I hope will throw ſome light on theſe queſtions. As it is to the accuracy of modern regiſters that we are originally indebted for our knowledge of the facts in queſtion, I apprehend, it is from the ſame ſource only that we ſhall be enabled ſatisfactorily to explain them.

Of the regiſtry incloſed, I beg leave to obſerve to you, Sir, that it has been kept from its commencement by a man of uncommon accuracy (one of the under-clerks of our Houſe of Commons); and that as the poor women and their children are obliged to paſs through his office, before leaving the Hoſpital, his ſituation is ſuch that there is no likelihood of his being deceived. It exhibits to our view the occurrences of 28 years in above 20,000 inſtances: a number which I am inclined to think can hardly appear inſufficient for eſtabliſhing ſome general inferences and concluſions on a tolerably ſure foundation. Although my reaſoning on theſe matters ſhould not appear very concluſive, or my calculations perfectly accurate, yet I flatter myſelf, that the facts will neither be unacceptable nor uſeleſs to you.

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* Vol. II. p. 247.

I believe it may be ſafely aſſerted, that anatomy has not hitherto detected any internal difference between the animal œconomy of the male and female, which can be ſuppoſed to account for their difference of mortality, more eſpecially in early Infancy; and this (it deſerves to be particularly remarked) is the period during which the chances are much the greateſt againſt male life. It is a matter of common obſervation that *males*, *cæteris paribus*, grow to a greater ſize than females, both *in utero* and every ſubſequent period of their growth. Conſequently, they muſt meet with more difficulty, and endure more hardſhip and fatigue, in the hour of birth. Accordingly, practitioners in midwifry, taught by experience, know, that when any conſiderable difficulty occurs in the birth of a child (for example, in all the different kinds of preternatural labours) they ſtand a much better chance of ſaving the life of a female than of a male. It is on this principle we can explain what our regiſtry concurs with others in proving, *viz.* that near one-half more males than females are ſtill-born. Naturaliſts are agreed, that the head of the human fœtus is larger in proportion to its body than that of any other animal; and I believe it is certain, that no animal whatever brings forth its young with ſo much difficulty, pain, and danger, as a woman. Now as we know that the head contains one of the moſt important organs of the body to life, it is highly reaſonable to ſuppoſe, that any additional injury which it ſuſtains in delivery may produce very material effects on the whole ſyſtem. Theſe effects though often may not be always immediate. They may operate in weakening the male conſtitution ſo as to render it more apt to be affected by any exciting cauſe of diſeaſe ſoon after birth, and leſs able to ſtruggle againſt it. It may be aſked, how this will apply to the difference of mortality in great towns and country ſituations? The anſwer evidently is, that in great towns rickets, ſcrophula, and other diſeaſes affecting the bones, and producing conſequent mal-conformation of the female ſex, are more frequent than in healthy country ſituations.

There is another circumſtance, Sir, which may have ſome influence in producing that particular *debility* which you mention. It is this: as the ſtamina of the male are naturally conſtituted to grow to a greater ſize, a greater ſupply of nouriſhment *in utèro* will be neceſſary to his growth than to that of a female. Defects in this particular, proceeding from delicacy of conſtitution or diſeaſes of the mother, muſt of courſe be more injurious to the male ſex. And although the male children may be ſo lucky as to eſcape abortion and the perils of delivery, it is probable, that they will be more apt to languiſh under diſeaſe, or die at ſome future period, from the application of noxious cauſes to an originally half-ſtarved frame. To a perſon little accuſtomed to conſider phyſiological ſubjects, this reaſoning may appear ſomewhat obſcure. It may, perhaps, be ſomewhat illuſtrated by conſidering that nouriſhment of the fœtus *after* birth which nature has provided for. Suppoſe every mother in a great city obliged to ſuckle and nurſe her own child, *without* the aſſiſtance of ſpoon-meat; and every mother in the adjacent country to do the ſame. Of the former there would not perhaps be one *good* nurſe in *five*; and of the

latter,

latter, perhaps, *not one* bad in *ten*. The difference of mortality that would enſue both to mothers and children thus ſituated, and the greater ſufferings of the male than female ſex, may be eaſily conceived, but not eaſily calculated. We ſee that, when a woman conceives twins, and has two fœtuſes *in utero* to nouriſh inſtead of one, it becomes peculiarly fatal both to her and her offspring. The chances are above four to one greater againſt her than againſt a woman bringing forth one child, and about two to one againſt her iſſue *.

Give me leave, Sir, to call your attention a little further to the facts relating to twins. They are ſingular and curious, at the ſame time that they ſerve to confirm ſome of the preceding reaſoning. Near *one-half* more twins die, and near *one-third* more are ſtill-born, than of ſingle children. And why?—It is not becauſe they meet with greater difficulties in the birth. On the contrary, it is a known fact, that, being much leſs than other children, women bring them forth with more eaſe. Does it not then proceed from a ſcanty nutrition, by which they are oftener blighted *in utero* than ſingle children; and, when born alive, have leſs ſtrength to ſupport life through the firſt ſtages of its exiſtence.

It is farther worthy of obſervation, that though *double* the numbers of twins die and are ſtill-born, compared to ſingle children, yet the proportion of male twins loſt to females is *leſs*. Only one-fifth more of the male ſex die than of the female, and only one-third more is ſtill-born. Whereas of ſingle children, whoſe proportional mortality is one-half leſs, *one-fourth* more of the male ſex die, and near double the number is ſtill-born. To what then are we to attribute this leſſened mortality in favour of male twins? Probably to their brain and nervous ſyſtem ſuffering leſs during delivery, on account of their heads being much ſmaller than thoſe of ſingle children. Were I diſpoſed to be prolix, I could offer many more plauſible arguments on this ſubject; but to you, Sir, I am ſure they would be unneceſſary. There is only one circumſtance remaining, relative to the proportion of the ſexes, which I cannot paſs over in ſilence. We ſee evident wiſdom in the creation of a greater number of males than females; but why the proportion they bear to each other differs in different countries and ſituations, and why there ſhould be a ſeventeenth more males born of ſingle children than twins, are queſtions which I leave to be decided by thoſe philoſophers who underſtand the theory of generation better than I do. Be this as it may, I am convinced that the majority in favour of the male ſex is ſooner deſtroyed than the generality of writers ſeem to be aware of. Did the limits of this letter permit, I think, I could prove from Dr. SHORT's own data*, that the majority of males is deſtroyed long before the common marriageable period; but I ſhall content myſelf with an obſervation or two on the regiſtry before us. If one-half of the whole born in this hoſpital die before three years, which is the eſtabliſhed computation for great cities; and if, on the loſs of ſomewhat more than a *third* of this half, a majority of 1177 be reduced to 483 by a loſs of 694, as appears from the regiſtry, it is pretty evident, that by the death of the two remaining thirds, a majority will be left in favour of the female ſex. It is obvious, that the ſtatement with regard to twins corroborates this ſuppoſition; for of them, inſtead of a fifth, there is near one *half* dead and ſtill-born, the conſequence of which is, that we ſend out a majority of females. It may be objected, that their males do not bear ſo great a proportion to the females; and that, therefore, it is not to be expected they ſhould keep up their majority ſo long. But there is only a ſeventeenth fewer males produced; whereas it has been already ſhewn, that there is a much greater proportion between the deaths of ſingle and twin males againſt the former and in favour of the latter.

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* Compare the 7th and 14th, 6th and 13th inferences in the annexed extracts.

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* New Obſervations, p. 72. et ſeq.

Such are the outlines, Sir, of my ſentiments on this ſubject. I have aſſumed the liberty of addreſſing them to you without ceremony, as a well-wiſher to every member of the republic of letters. I ſhall be happy, ſhould your ſentiments happen to coincide with mine, or if I can be of any farther ſervice in promoting your very laudable inquiries.

I am, Sir, with great reſpect, &c.

JOSEPH CLARKE.

Lying-in Hoſpital,
June 9, 1785.

Dr. Clarke's *ſecond Letter to the Rev. Dr.* Price.

SIR,

Dublin, Oct. 22, 1785.

ENCOURAGED by your approbation of my former letter, I will take the liberty of ſtating to you a few more facts and obſervations, which I hope you will judge an Appendix to it of ſome importance.

With the view of aſcertaining how far ſome of the foregoing conjectures are well founded, and of determining with greater preciſion the more obvious differences between the male and female ſex in infancy, I began in the month of July laſt by weighing forty children, twenty of each ſex, and by taking the dimenſions of their heads. In the months of Auguſt and September I repeated the ſame experiment twice, taking ſuch children as appeared to have arrived at the full period of geſtation promiſcuouſly as they happened to be born.

I weighed them all a few hours after birth, before they had taken food, and before purgative medicines had time to operate. For this purpoſe, I made uſe of a ſmall ſpring or pocket ſteelyard, which weighs any thing (not heavier than a few pounds) appended to it with ſufficient accuracy. To this was attached a flannel bag, into which the children were put, at firſt, naked; but this I ſoon found very troubleſome. The nurſes often wanted time ſufficient to aſſiſt me, and timid mothers were afraid of their infants catching cold; I was therefore obliged to weigh them with their cloaths on, and to ſubtract a certain quantity from the groſs weight of each child, according as it was full, middling, or light cloathed. Whatever inaccuracy this may have introduced, as to the real weight of the children, it can but little influence their comparative weights, or the differences between the two ſexes, which it was my object to aſcertain.

For meaſuring their heads, I made uſe of a piece of painted or varniſhed linen tape, divided into inches, halves, and quarters. The varniſh has the good effect of preventing the length of ſuch a meaſure being readily affected by variations in the humidity of the atmoſphere, &c.; and it has little or no elaſticity. In this part of the experiment then I can pretend to conſiderable accuracy. I took firſt the greateſt circumference of the head from the moſt prominent part of the occiput around over the frontal ſinuſes; and, ſecondly, the tranſverſe dimenſion from the ſuperior and anterior part of one ear, acroſs the fontanelle, to a ſimilar part of the oppoſite ear. Theſe dimenſions appeared to me the moſt likely to afford data for determining the reſpective ſizes of the brain in the different ſexes. The reſult was as follows:

Twenty males.			Twenty females.		
Weight. lbs. &c.	Circumference of heads. Inches.	Dimenſions from ear to ear. Inches.	Weight. lbs. &c.	Circumf. of heads. Inches.	Dimen. from ear to ear. Inches.
		Experiment 1.			
149½	282	152	137¼	272	143
		Experiment 2.			
144½	277	146¼	135	272	147
		Experiment 3.			
148	280	147½	132	273	143¼
		Totals.			
442	839	445¾	404¼	817	433¼
		Average weight, &c.			
7 lbs. 5 oz. 7 dr.	14	7¼	6 lbs. 11 oz. 6 dr.	13⅝	7²⁄₉

Having found the relative proportions between the ſexes to turn out thrice with ſo much uniformity, and obſerving them to correſpond pretty nearly with ſome experiments, made for very different purpoſes by the late Profeſſor Roederer, of Gottingen, I did not think it neceſſary to proſecute the ſubject farther.

Upon the whole, it may be obſerved, that the difference of weight between the male and female at birth may be rated at about nine ounces, or nearly a twelfth part of the original weight. In the circumference of their heads there is a difference of near half an inch, or about a 28th or 30th part; and the ſame proportion of a 28th is pretty nearly preſerved in the tranſverſe dimenſion. It is evident, as the bony paſſage through which infants paſs is of a certain determined capacity, that, were their heads equally incompreſſible with thoſe of adults, the difference of half an inch in their ſize would often prove fatal to them. By the compreſſibility of their heads, however, in *well formed* women, this difficulty is by time ſurmounted. The effects which ſuch a compreſſion on the *brain* may produce, have not hitherto been well attended to.

In reckoning children, weighing from 5½ to 6½, 6 pounds weight, and from 6½ to 7½, 7, and ſo forth, in order to avoid fractions, I find the numbers of males and females, arranged according to their weight, to ſtand as follow.

Males.								Females.							
lbs.	4	5	6	7	8	9	10	lbs.	4	5	6	7	8	9	10
N°	0	3	6	32	16	2	1	N°	2	9	14	25	8	2	0

Hence it appears, that the majority of males runs thus: ſeven, eight, ſix, five; whilſt that of the females is ſeven, ſix, five, eight. Hence alſo appears the merciful diſpenſations of Providence towards the female ſex; for when deviations from the medium ſtandard occur, it is remarkable, that they are much more frequently below than above this ſtandard. In 120 inſtances there are only five children exceeding eight pounds and a half in weight. The ſame may be obſerved with regard to the ſize of their heads. Only ſix meaſured above 14½ inches in circumference, and theſe all of the male ſex; five meaſured 14¾, and one 15. In tranſverſe dimenſions only four exceeded 7¾, the largeſt of which was 8½; whereas deviations under the ſtandard in theſe particulars were very numerous, never however under 12 around and 6¼ acroſs.

In the year 1753, Dr. ROEDERER publiſhed a Paper, *De pondere et longitudine Infantum recens natorum*, in the Commentaries of the Royal Society of Gottingen, of which the celebrated HALLER was the principal inſtitutor, and long the preſident. In this Paper he proves, in the cleareſt manner, by inconteſtible experiments, the abſurdity of the ideas of obſtetric writers with regard to the progreſs of the ovum during geſtation, and the weight of the fœtus after birth. He ſhews, although they ſtate the weight of the fœtus, come to the full time, to be from 12 to 14 or 16 pounds, that it is more generally 6 or 7, and very rarely exceeds eight. This deſerves particular notice for two reaſons; firſt, becauſe it ſerves to ſhew how little dependence is to be placed on the aſſertions of authors who copy each other ſervilely, without having recourſe to experiment even in the moſt obvious caſes; and, ſecondly, becauſe this paper has been overlooked by ſome of the moſt celebrated writers and teachers of midwifry now living. What idea are we to form of the accuracy of one of our lateſt ſyſtematic writers, who (telling us that he has been a practitioner of midwifry, in a capital city, for twenty years, and a teacher for more than twelve) ſtates, in one page of his work, that the weight of a fœtus at eight months is about ſeven pounds; and on the oppoſite page, that at full time it weighs from twelve to fourteen pounds *?

Of 27 children, carried to the full period of geſtation, weighed and meaſured in length by ROEDERER, without any attention to the difference of ſex, I find, that 18 were of the male and 9 of the female ſex; and that the average weight of

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* See a Treatiſe of Midwifry (p. 88. and 89.) diveſted of *technical* terms and *abſtruſe theories*, by A. HAMILTON, M. D. 8° edit. London, 1781.

the former was about 6 lbs. 9 oz., that of the latter about 6 lbs. 2 oz. 2 dr. Whether he and I uſed the ſame weights, I cannot exactly ſay. He obſerves, that he uſed the civil pound of Gottingen, which I can eaſily perceive conſiſted of 16 ounces, as mine did; but whether a German ounce be the ſame with ours, I have not *data* to determine. The average length of the males meaſured by him is about $20\frac{1}{3}$ inches, and of the females about $19\frac{17}{18}$. He weighed alſo the placentæ of 21 lying-in women, 16 of whom had borne male children, and five female. The average weight of the former was 1 lb. $2\frac{1}{2}$ oz.; that of the latter 1 lb. 2 oz. Hence it appears, that in other circumſtances, beſides thoſe I have taken notice of, the male and female ſex differ. So far I thought it neceſſary to take extracts from Dr. ROEDERER's paper, as his obſervations and mine throw light on each other, and add confirmation to both.

The limits of this letter will not permit me, Sir, to treſpaſs much farther on your patience. There is one circumſtance or two ſo intimately connected with my former letter, that I cannot paſs them over in ſilence. Having found that males ſuffer more in the birth than females, I was deſirous of knowing whether the chance of the mother's recovery was thereby in any degree affected; and to determine this I was once more at the pains of turning over our regiſtry with care. I found, that of 214 women, dead of ſingle children, 50 were delivered of ſtill-born males, and 15 of ſtill-born females; 76 of living males, and 73 of living females. Of the 15 dead of twins, 6 had twins one of each ſex; 6 others had twins both of the male ſex; and three had twins both of the female ſex. All of which twins (two or three excepted), it is very remarkable, ſurvived the death of their mothers. It would appear then, that the life of the mother is principally endangered in thoſe caſes where the bulk of the male's head precludes the poſſibility of his being brought into the world alive, either by the efforts of nature or art. The conception of twins we have obſerved to be more fatal to the mother than that of ſingle children. The average weight of 12 twins, which have occurred to me of late, I find to be 11 lbs. a pair. The largeſt pair weighed 13 lbs. and the leaſt 8½. From ſome rude attempts made to aſcertain the weight of the contents of the gravid uterus in caſes of twin and ſingle children, I am inclined to think, that they are to each other as about 15 to 10, or perhaps 14½ to 9½.

Believe me, Sir, with great reſpect, &c.

J. CLARKE.

Year ending 31 of December,	1771	724	29	695	370	341	711	16	102	44	5
	1772	725	21	704	368	344	712	8	116	32	4
	1773	727	33	694	367	344	711	17	136	31	1
	1774	709	28	681	357	334	691	10	154	29	2
	1775	752	24	728	364	378	742	14	122	27	5
	1776	883	31	802	418	407	825	22	132	39	7
	1777	872	37	835	452	395	847	1 had 3 12	145	35	7
	1778	961	34	927	476	460	936	9	127	39	10
	1779	1064	53	1011	550	476	1026	15	146	59	8
	1780	967	48	919	499	441	940	21	115	41	5
	1781	1079	52	1027	598	447	1045	18	121	38	6
	1782	1021	31	990	549	458	1007	17	127	57	6
	1783	1230	63	1167	632	553	1185	17	91	72	15
	1784	1317	57	1260	642	640	1282	1 had 3 23	76	68	11
Totals		20625	839	19786	10647	9470	20117	331	3111	1006	229

. Proportion of males and females born, about *nine* males to *eight* females.
children dying under *ſixteen* days old, as *one* to about *ſix and a half.*
children ſtill-born, as *one* to about *twenty.*
women having twins, as *one* to about *ſixty.*
women dying in child-bed, as *one* to about *eighty-ſeven.*

An Abſtract of the Regiſtry kept at the Lying-in Hoſpital, in Dublin, from the 8th of December, 1757, to th 31ſt of December, 1784. By B. H. Regiſter.

		Number of Patients admitted	Went out not delivered	Delivered in the Hoſpital.	Boys born.	Girls born.	Total number of children.	Women having twins.	Children dead.	Children ſtill-born	Women dead.
From 8th to 31ſt of December,	1757	55	– –	55	30	25	55	– –	6	3	1
Year ending 31 of December,	1758	455	1	454	255	207	462	8	54	21	8
	1759	413	7	406	228	192	420	13 1 had 3	95	22	5
	1760	571	15	556	300	260	560	4	116	36	4
	1761	537	16	521	283	249	532	11	104	29	9
	1762	550	17	533	279	266	545	12	106	33	6
	1763	519	31	488	274	224	498	12	94	29	9
	1764	610	22	588	287	308	595	7	83	28	12
	1765	559	26	533	288	251	539	6	94	25	6
	1766	611	30	581	324	261	585	4	111	18	3
	1767	695	31	664	373	301	674	10	125	29	11
	1768	689	34	655	362	302	664	9	154	47	16
	1769	675	33	642	350	301	651	9	152	38	8
	1770	705	35	670	372	305	677	7	107	37	8
	1771	724	29	695	370	341	711	16	102	44	5
	1772	725	21	704	368	344	712	8	116	32	

Extracts from the Regiſtry kept at the Lying-in

Uniparous.

Women.		Children.					
Delivered in Hoſpital.	Dead.	Sex.		Dead.		Still-born.	
		M.	F.	M.	F.	M.	F.
19455	214	10305	9150	1656	1247	602	351
		9150		1247		351	
		19455		2903		953	
				953			

Total 3856 dead and ſtill born.

Inferences.

1.	Proportion of males to females born nearly as	17 to 15
2.	——— children dying under 16 days	1 to 6⅔
3.	——— children ſtill-born	1 to 20⅖
4.	——— males dying to females	4 to 3
5.	——— ——— ſtill-born to ditto	12 to 7
6.	——— ſtill-born and dead of each ſex to the whole	1 to 5
7.	——— women dying in child-bed	1 to 92

Totals of dead and ſtill-born.

	Males.	Females.
	1656	1247
	602	351
	2258	1598
Born in hoſpital	10305	9150
Dead and ſtill-born	2258	1598
Sent out living	8047	7552
	7552	
Balance	495 in favour of the male ſex.	

Totals of dead and ſt

uniparous or mu

	Males.
	1656
	116
	602
	29
	2403
Born	10647
	2403
	8244
	7761

Of 20117 children born, at the end of a fortnight, there is only a balance of 483 in fa

g-in Hoſpital, Dublin, from the year 1757 to 1784.

Multiparous, Twins, Triplets, &c.

Women.		Children.					
Delivered in Hoſpital.	Dead.	Sex.		Dead.		Still-born.	
		M.	F.	M.	F.	M.	F.
331	15	342	320	116	91	29	20
		320		91		20	
		662		207		49	
				49			
			Total	256	dead and ſtill-born.		

Inferences.

8. Proportion of male twins to females born . . 17 to 16

9. ——— twins dying under 16 days . . 1 to $3\frac{1}{5}$

10. ——— twins ſtill-born . . . 1 to $13\frac{1}{2}$

11. ——— male twins dying to females . . 5 to 4

12. ——— —— —— ſtill-born to ditto . . 3 to 2

13. ——— ſtill-born and dead of each ſex to the whole 1 to $2\frac{3}{5}$

14. ——— women dying . . . 1 to 22

d and ſtill-born whether
s or multiparous.

s. .	Females.
	1247
	91
	351
	20
	1709
	9470
	1709
	7761

Totals of twins, &c. dead and ſtill-born.

	Males.	Females.
	116	91
	29	20
	145	111
Born	342	320
Dead and ſtill-born	145	111
Sent out living	197	209
		197
Balance in favour of the female ſex	12	

g in favour of the male ſex, although originally 1177; greater loſs of males 694.

XVIII. *Some Particulars of the preſent State of* Mount Veſuvius; *with the Account of a Journey into the Province of* Abruzzo, *and a Voyage to the Iſland of* Ponza. *In a Letter from Sir* William Hamilton, *K. B. F. R. S. and A. S. to Sir* Joſeph Banks, *Bart. P. R. S.*

Read May 4, 1786.

SIR, Naples, January 24, 1786.

THE eruption of Mount Veſuvius, which began in the month of November 1784, nearly at the moment of my return from England to this Capital, and which continued in ſome degree till about the 20th of laſt month, has afforded much amuſement to travellers unacquainted with this wonderful operation of nature, but no new circumſtance that could juſtify my troubling you with a letter on the ſubject. The lava either overflowed the rim of the crater, or iſſued from ſmall fiſſures on its borders, on that ſide which faces the mountain of Somma, and ran more or leſs in one, and at times in three or four channels, regularly formed, down the flanks of the conical part of the volcano; ſometimes deſcending and ſpreading itſelf in the valley between the two mountains; and once, when the eruption was in its greateſt force, in the month of November laſt, the lava deſcended ſtill lower, and did ſome damage to the vineyards, and cultivated parts at the foot of Veſuvius, towards the village of St. Sebaſtiano; but generally the lava, not being abundant, ſtopped and cooled before it was able to reach the valley. By the accumulation of theſe lava's on the flanks of Veſuvius, its form has been greatly altered; and by the frequent exploſion of ſcoriæ and aſhes, a conſiderable mountain has been formed within the crater, which now riſing much above its rim has likewiſe given that part of the mountain a new appearance. Juſt before I left Naples, in May 1783, I was at the top of Veſuvius. The crater was certainly then more than 250 feet deep, and was impracticable, its ſides being nearly perpendicular. This eruption, however, has been as ſatisfactory as could be deſired by the inhabitants of this city, a prodigious quantity of lava having been diſgorged; which matter, confined within the bowels of the earth, would probably have occaſioned tremors; and even ſlight ones might prove fatal to Naples, whoſe houſes are, in general, very high, ill built, and a great number in almoſt every ſtreet already ſupported by props, having either ſuffered by former earthquakes, or from the looſe volcanic ſoil's having been waſhed from under their foundations by the torrents of rain water from the high grounds which ſurround Naples, and on which a great part of the town itſelf is built.

From the time of the laſt formidable eruption of Mount Veſuvius, in Auguſt 1779 (deſcribed in one of my former communications to the Royal Society) to this day, I have, with the aſſiſtance of the Father Antonio Piaggi *, kept an exact diary of the operations of Veſuvius, with drawings, ſhewing, by the quantity of ſmoke, the degrees of fermenta-

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* This Padre Antonio Piaggi is the ingenious Monk who invented the method of unfolding and recovering the burnt ancient manuſcripts of Herculaneum, and who reſides conſtantly at Reſina, at the foot, and in full view, of Mount Veſuvius.

tion of the volcano; alſo the courſe of the lava's during this laſt eruption, and the changes that have been made in the form of the mountain itſelf by the lava's and ſcoriæ that have been ejected. This journal is becoming very curious and intereſting; it is remarkably ſo with reſpect to the pointing out a variety of ſingular effects that different currents of air have upon the ſmoke that iſſues from the crater of Veſuvius, elevated (as you know, Sir) more than 3600 feet above the level of the ſea; but, except the ſmoke increaſing conſiderably and conſtantly when the ſea is agitated, and the wind blows from that quarter, the operations of Veſuvius appear to be very capricious and uncertain. One day there will be the appearance of a violent fermentation, and the next all is calmed again: but whenever the ſmoke has been attended with conſiderable ejections of ſcoriæ and cinders, I have conſtantly obſerved, that the lava has ſoon after made its appearance, either by boiling over the crater, or forcing its paſſage through crevices in the conical part of the volcano. As long as I remain in this country, and have the neceſſary aſſiſtance of the above-mentioned ingenious Monk (who is as excellent a draughtſman as he is an accurate and diligent obſerver) the Veſuvian diary ſhall be continued; and I hope one day to have the honour of preſenting theſe curious manuſcripts (which begin now to be voluminous) to the Royal Society, if it ſhould think them worthy of a place in the Library of the Society.

Having never had an opportunity of examining the iſlands of Ponza, Palmarole, Zannone, and other ſmall iſlands, or rather rocks, ſituated between the iſland of Ventotiene and Monte Circello, near Terracina, on the Continent; and thinking that by a tour of theſe iſlands I ſhould be enabled to render my former obſervations more complete, and to communicate to you, Sir, ſome account of the only volcanic parts of this neighbourhood hitherto undeſcribed, I determined to take advantage of the abſence of their Sicilian Majeſties (who were then making the tour of Italy) and viſit theſe iſlands. But before I put this plan in execution, I made a long excurſion in the province of Abruzzo, as far as the Lake of Celano, anciently called Fucinus, and where the famous Emiſſary of the Emperor CLAUDIUS (a moſt ſtupendous work * for draining that lake) remains nearly entire, though filled up with rubbiſh and earth in many parts, and of courſe uſeleſs. The water of this lake, which is more than 30 miles in circumference, increaſes daily, and is deſtroying the rich and cultivated plains on its borders. It is ſurrounded by very high mountains, many of them covered with ſnow, and at the foot of them are many villages, and rich and well cultivated farms. Upon the whole it is the moſt beautiful lake I ever ſaw, and would be complete, if the neighbouring mountains were better wooded. This lake furniſhes abundance of fiſh, but not of the beſt quality: a few large trout, but moſtly tench, barbel, and dace. In the ſhallow water on the borders of the lake, I ſaw thouſands of water ſnakes, purſuing and preying upon a little fiſh like our thornbacks, but much better armed, though their defenſive weapons ſeemed to avail them but little againſt ſuch ravenous foes.

I went with torches into the emiſſary of CLAUDIUS as far as I could. It is a covered under-ground canal, three miles long, and great part of it cut through a hard rock; the other parts ſupported by maſonry, with wells ſunk to give air and light.

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* A deſcription of this emiſſary of CLAUDIUS, with plans (though not very exact) has been publiſhed by FABRETTI, in the ſame book in which he has given an account of TRAJAN's column.

According to SUETONIUS, CLAUDIUS employed thirty thouſand men eleven years on this great work, intended to convey the ſuperfluous water of the lake into the bed of the river Liris, now called Garigliano; and I make no doubt, but that if it was cleared and repaired, it would again anſwer that purpoſe.

In its preſent ſtate it is a moſt magnificent monument of antiquity.

The whole country from Arpino, the native place of MARIUS*, by Iſola, Sora, Civitella, and Capiſtrello, to the lake of Celano, is, in my opinion, infinitely more beautiful and picturesque than any ſpot I have yet ſeen on the Alps, in Savoy, Switzerland, or the Tyrol. The road is not paſſable for carriages, and indeed is ſcarcely ſo, even in ſummer, for horſes or mules, and is often infeſted with banditti; a party of which, conſiſting of twenty-two, had quartered themſelves in a village which I paſſed through, and left it but a week before my arrival. There are many wolves and ſome bears in the adjacent mountains, which alſo commit their depredations in the winter. The tyger-cat, *gatto pardo*, or lynx, is ſometimes found in the woods of this part of Abruzzo.

The road follows the windings of the Garigliano, which is here a beautiful clear trout ſtream, with a great variety of caſcades and water-falls, particularly a double one at Iſola, near which place CICERO had a villa, and there are ſtill ſome remains of it, though converted to a chapel. The valley is extenſive, and rich with fruit trees, corn, vines, and olives. Large tracts of land are here and there covered with woods of oak and cheſtnut, all timber trees of the largeſt ſize. The mountains neareſt the valley riſe gently, and are adorned with either modern caſtles, towns, and villages, or the ruins of ancient ones. The next range of mountains, riſing behind theſe, are covered with pines, larches, and ſuch trees and ſhrubs as uſually abound in a like ſituation: and above them a third range of mountains and rocks, being the moſt elevated part of the Apennine, riſe much higher, and, being covered with eternal ſnow, make a beautiful contraſt with the rich valley above-mentioned; and the ſnow is at ſo great a diſtance, as not to give that uncomfortable chill to the air, which I have always found in the narrow vallies of the Alps and the Tyrol. Excuſe me, Sir, if from the impreſſion which this enchanting and little frequented country has left on my mind, I have been led to depart from the ſubject of this letter, to which I will return directly.

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* MARIUS had a large villa, about twelve miles diſtant from Arpino. I went to viſit the ſpot, on which now ſtands the only convent of the auſtere order of La Trappe in Italy. It is in the Pope's ſtate, and has been evidently built of the ruins of MARIUS's houſe, and its preſent name is *Caſa Mari*.

On the 15th of Auguſt laſt I went in a felucca to the iſland of Iſchia. I have nothing to add to my former obſervations on this iſland, already communicated to the Royal Society; except that about ſixty yards from the ſhore, at a place called St. Angelo, ſituated between the towns of Iſchia and Furia, a column of boiling water bubbles upon the ſurface of the ſea with great force, and communicates its heat to the water of the ſea near it; but as the wind was very high, and the ſurf conſiderable, I was not able then to examine this curious ſpot as I could have wiſhed, but will return there on purpoſe ſome other time. The inhabitants of the neighbourhood told me, that it always boiled up in the ſame manner, winter and ſummer; and that it was of great uſe to them in bending their planks for ſhip-building; and that the fiſhermen alſo frequently made uſe of this natural cauldron to boil their fiſh. Though I have paſſed at different times many weeks in the iſland of Iſchia, I never before heard of this phænomenon; but in my deſcription of this iſland mention is made of ſeveral ſpots where, near the ſhore, I had found, when bathing in the ſea, the ſand under my feet ſo hot as to oblige me to retire haſtily. This boiling ſpring reminds me of one near Viterbo in the Roman State, which I have ſeen, and is called the Bulicame. It is a circular pool of about ſixty feet in diameter, and exceedingly deep, the water of which is conſtantly boiling. It is ſituated in a plain ſurrounded by volcanic mountains. A ſtony concretion floats on the ſurface of the pool, which being carried off by the ſuperfluous water is depoſited, and is conſtantly forming a labes or tuffa, of which all the ſoil around the pool is compoſed. You have ſeen, Sir, the like operation in greater perfection in Iceland, at the famous boiling ſpring of Geyſer. I am convinced, that many of the finer ſort and moſt compact tuffa's we meet with, in countries formed by volcanoes, have been produced in the ſame manner.

The 18th of Auguſt I arrived at the iſland of Ventotiene, about twenty-five miles from Iſchia. It is greatly improved ſince my former viſit, ſeven or eight years ago, when his Sicilian Majeſty firſt planted a little colony there. It then produced neither corn nor wine; now it furniſhes annually at leaſt ſeventy butts of wine and two thouſand *tomoli* of corn. The ſoil is remarkably fertile, from whence it probably took its ancient Greek name of Pandataria. This iſland contains at preſent more than three hundred inhabitants. The iſland of Ventotiene, and the ſmaller one called St. Stefano, within a mile of it, having been deſcribed in my Campi Phlegræi, as being both entirely compoſed of volcanic matter, I need not trouble you further on their ſubject; I will only mention a

curious

curious circumſtance in the natural hiſtory of birds, of which I was informed by an officer of the garriſon of Ventotiene, who is a great ſportſman, and ſhoots often in the iſland of St. Stefano, inhabited only by hawks, and a large kind of ſea-gulls; but is occaſionally viſited, as a reſting place, by divers ſorts of birds of paſſage. In the month of May great flights of quails arrive there from Africa, ſpent with fatigue; and many of them fall an eaſy prey for the hawks and ſea-gulls; but, as their arrival depends upon one prevailing wind, there is often an interval of many days between one flight and another. My informer aſſured me, that the hawks conſtantly, during the flights, make a proviſion of each day's prey, laying them up in ſeparate heaps of ſix or ſeven near their haunts, always feeding firſt upon thoſe of the oldeſt date. The ſea-gulls have not the ſame foreſight, but greedily fall upon their unhappy victims in their languid ſtate before they reach the ſhore, and, having beat them down into the ſea, ſwallow numbers of them whole. Extraordinary as this may appear, yet as facts related by perſons of credibility in any branch of natural hiſtory are always pleaſing, I thought you would excuſe this digreſſion. Give me leave likewiſe to add, for the information of the curious in antiquities, that, during my ſtay in the iſland of Ventotiene, I got out of the ruins of an elegant ancient bath (ſuppoſed to have been built for the uſe of JULIA, daughter of AUGUSTUS, whilſt ſhe was in exile here) a fragment of a tile, on which are ſtamped the following characters in baſſo relievo,

HACINI
IVLIAI
AVGVS. F

which, according to the interpretation of a celebrated antiquary at Naples, mean *Opus* HACINI *ad commodum Balnei* JULIÆ AUGUSTÆ *factum*. I was informed, that ſeveral entire tiles, with a like inſcription, had been dug up on the ſame ſpot, and had been made uſe of in building the church and barracks newly erected in this iſland. Another fragment of a tile was likewiſe found here, and given to me, with the following inſcription:

SAB. A PI.

which the ſame antiquary explains, SABINAE AVGVSTAE, Piæ Imperatrici *dicatum Balneum*; but, I believe, there is no mention in ancient authors of SABINA having been at Pandataria: of JULIA's baniſhment to this iſland there can be no doubt.

Between Ventotiene and the iſland of Ponza, and from the latter at the diſtance of about twelve miles, a group of rocks riſe ſeveral feet above the ſurface of the ſea. They are called the *Botte*, and are compoſed of a compact lava; probably they are the ſmall remains of another volcanic iſland, the ſofter parts of which may have been carried off and levelled by the action of the ſea, which is open and violent here.

The 20th of Auguſt I arrived at the iſland of Ponza, about thirty miles from Ventotiene, and the next day I went round it in my boat. It is near five miles long: its greateſt breadth not more than half a mile, and in ſome parts not more than five hundred feet. It is ſurrounded by innumerable detached rocks, ſome of them very high, and moſt of which are of lava; in many are regularly formed baſaltes, but none in large columns. In ſome parts the baſaltes have a reddiſh tint of iron ochre, are very ſmall, and irregularly laid one over another. Some maſſes of them are in a perpendicular, others in an horizontal, and others again in an inclined poſition: and the rocks themſelves, in which theſe maſſes are found, are lava of the ſame nature as the baſaltes. At firſt ſight theſe rocks have very much the appearance of the ruins of ancient Roman brick or rather tile buildings, as may be ſeen in the drawing (ſee Tab. XI. fig. 1.) taken on the ſpot. One rock, as appears in the drawing (ſee Tab. XII. fig. 4.) is compoſed of large ſpherical baſaltes; and in many parts of the iſland I found the lava had inclined to take the like ſpherical form, though on a much ſmaller ſcale, ſome of the firſt mentioned round baſaltes being near two feet in diameter. All theſe rocks have certainly been detached by the action of the ſea from the iſland, which is intirely compoſed of volcanic matter, lava's, and tuffa's, of various qualities and tints, green, yellow, black, and white. Some of the tuffa's, as well as the lava's, are of a texture more compact than others; and in ſome parts of the iſland great tracts ſeem to have undergone the ſame operation as is mentioned in one of my former communications to be in full force at a ſpot called the Piſciarelli, on the outſide of the Solfaterra, near Puzzole, and where a hot ſulphureous vitriolic acid vapour converts all which it penetrates, whether lava's, tuffa's, volcanic aſhes, or pumice ſtones, into a pure clay, moſtly white, or with a light tint of red, blue, green, or yellow. The appearance of a tract of volcanic country, which has undergone this operation, is well expreſſed in the view of the inſide of the harbour of Ponza (Tab. XI. fig. 2.). But I was ſo ſtruck with the beautiful and uncommon appearance of one of theſe high volcanic grounds converted to a pure light-coloured clay (Tab. XII. fig. 1.) in contraſt with a neighbouring dark baſaltic rock, that I cauſed the drawing, which accompanies this letter (ſee Tab. XII.) to be made on the ſpot. You, Sir, who have ſeen ſuch a variety of countries, will ſtill think this view ſingular and beautiful. I can aſſure you, it is very exact, except the rock of round baſaltes (fig. 4.) which, in nature, is at a diſtance from this ſpot, and only placed here to illuſtrate what I have written on its ſubject. In one part of the iſland there is a ſort of tuffa, remarkably good for the purpoſe of building. It is as hard as our Bath ſtone, and nearly of the ſame colour, without any mixture of fragments of lava or pumice ſtone, which uſually abound in the tuffa's in the neighbourhood of Naples, Baia, and Puzzole.

The drawing (ſee Tab. XI.), which is a view of the harbour of Ponza, will give you a very good idea of the appearance of the iſolated rocks of lava and baſaltes which have been ſeparated, by the force of the ſea, from the ſofter parts of the iſland, and of which there are an infinite number, as you will ſee in the exact geometrical plan of the iſland of Ponza (Tab. X.), which likewiſe accompanies this letter.

When I was laſt in England, I inquired of many of the manufacturers of glaſs, whether it had ever happened, that the glaſs cooling in their furnaces had taken any diſtinct forms like priſms or cryſtallizations; but I got no ſatisfactory anſwer until I applied to the ingenious Mr. PARKER, of Fleet-ſtreet, who not only informed me, that, ſome years ago, a quantity of his flint glaſs had been rendered unſerviceable by taking ſuch a form in cooling; but alſo gave me ſeveral curious ſpecimens of the glaſs itſelf: ſome of them are in laminæ, which may be eaſily ſeparated; and others reſemble baſaltic columns in miniature, having regular faces. I was much pleaſed with this diſcovery, proving to me, beyond a doubt, the volcanic origin of moſt baſaltes. Many of the rocks of lava of the iſland of Ponza are, with reſpect to their configurations, ſtrikingly like the ſpecimens of Mr. PARKER's above-mentioned glaſs, none being very regularly formed baſaltes, but all having a tendency towards it. Mr. PARKER could not account for the accident that occaſioned his glaſs to take the baſaltic forms; but I have remarked, both in Sicily and at Naples, that ſuch lava's as have run into the ſea, are either formed into regular baſaltes, or have a great tendency towards ſuch a form. The lava's of Mount Etna, which ran into the ſea near Iacci, as appears in my account of them in the Campi Phlegræi, are perfect baſaltes; and a lava that ran into the ſea from Mount Veſuvius, near Torre del Greco, in 1631, has an evident tendency to the baſaltic forms. On Mount Veſuvius I never found any thing like columns of baſaltes, except the abovementioned at Torre del Greco, and ſome fragments of very complete ones, which I picked up near the crater, after the eruption of 1779, and which had been thrown out of the mouth of the volcano.

The iſland of Palmarole, which is about four miles from Ponza, is not much more than a mile in circumference, is compoſed of the ſame volcanic matter, and probably was once a part of Ponza; and indeed it appears as if the iſland of Zannone, which lies at about the ſame diſtance from the iſland of Ponza, was once likewiſe a part of the ſame iſland of Ponza; for many rocks of lava riſe above water in a line between the two laſt mentioned iſlands, and the water is much ſhallower there than in the other parts of the gulph of Terracina.

The iſland of Zannone is larger and much higher than Palmarole, and the half of the iſland neareſt the Continent is compoſed of a lime-ſtone, exactly ſimilar to that of the Apennines, on the Continent near it; the other half is compoſed of lava's and tuffa's, reſembling in every reſpect the ſoil of the other iſlands juſt deſcribed. Neither Palmarole nor Zannone are inhabited; but the latter furniſhes bruſhwood in abundance for the uſe of the inhabitants of Ponza, whoſe number, including the garriſon, amounts to near ſeventeen hundred.

The uninhabited iſland of St. Stefano furniſhes fuel in the like manner for the inhabitants of Ventotiene.

It is probable, that all theſe iſlands and rocks may in time be levelled by the action of the ſea. Ponza, in its preſent ſtate, is the mere ſkeleton of a volcanic iſland, as little more than its harder vitrified parts remain, and they ſeem to be ſlowly and gradually mouldering away. Other new volcanic iſlands may likewiſe be produced in theſe parts.

The gulphs of Gaeta and Terracina may, in the courſe of time, become another Campo Felice*; for, as has been mentioned in one of my former communications on this ſubject, the rich and fertile plain ſo called, which extends from the bay of Naples to the Apennines, behind Caſerta and Capua, has evidently been intirely formed by a ſucceſſion of ſuch volcanic eruptions. Veſuvius, the Solfaterra, and the high volcanic ground, on which great part of this city is built, were once probably iſlands; and we may conceive, the iſlands of Procita, Iſchia, Ventotiene, Palmarole, Ponza, and Zannone, to be the outline of a new portion of land, intended by nature to be added to the neighbouring Continent; and the Lipari iſlands (all of which are volcanic) may be looked upon in the ſame light with reſpect to a future intended addition of territory to the iſland of Sicily. If you caſt your eye, Sir, on the map at the head of my deſcription of the Campi Phlegræi, you will better underſtand my meaning.

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* The governor of the caſtle of Ponza, who has reſided there fifty-three years, told me, that the iſland was ſtill ſubject to earthquakes; that there had been one violent ſhock there about four years ago; but that the moſt violent one he ever felt there was on the very day and hour of the great earthquake which deſtroyed Liſbon; that two houſes out of three, which were then on the iſland, were thrown down. This ſeems to prove, that the volcanic matter, which gave birth to theſe iſlands, is not exhauſted.

The more opportunities I have of examining this volcanic country, the more I am convinced of the truth of what I have already ventured to advance, which is, that volcanoes ſhould be conſidered in a creative rather than a deſtructive light. Many new diſcoveries have been made of late years, particularly, as you well know, Sir, in the South-Seas, of iſlands which owe their birth to volcanic exploſions; and ſome, indeed, where the volcanic fire ſtill operates. I am led to believe, that upon further examination, moſt of the elevated iſlands at a conſiderable diſtance from Continents would be found to have a volcanic origin; as the low and flat iſlands appear in general to have been formed of the ſpoils of ſea productions, ſuch as corals, madrepores, &c. But I will ſtop here, and not deviate from the plan which I have hitherto ſtrictly followed, of reporting faithfully to my learned Brethren of the Royal Society ſuch facts only as come immediately under my own obſervation, and as I think may be worthy of their notice, and leave them at full liberty to reaſon upon them.

We may flatter ourſelves, as a very great progreſs has been made of late years in the knowledge of volcanoes, that by combining ſuch obſervations as we are already in poſſeſſion of, with thoſe which may be made hereafter, in the four quarters of the world (in all of which nature ſeems to have operated in a like manner), a much better theory of the earth may be eſtabliſhed than the miſerable ones that have hitherto appeared.

Thoſe who have not had an opportunity of examining a volcanic country, as I have for more than twenty years, would little ſuſpect, that many curious productions and combinations of lava's and tuffa's were of a volcanic origin; eſpecially when they have undergone various chemical operations of nature, ſome of which, as I have mentioned in a former communication, as well as in this, have been capable of converting tuffa's, lava's, and pumice ſtone, into the pureſt clay.

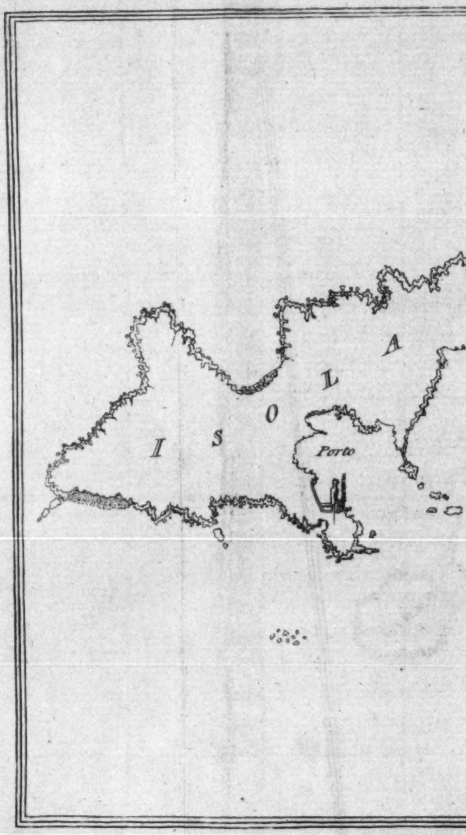
I have remarked, that young obſervers in this branch of natural hiſtory are but too apt to fall into the dangerous error of limiting the order of nature to their confined ideas: for example, ſhould they ſuſpect a mountain to have been a volcano, they immediately climb to its ſummit to ſeek for the crater, and if they neither find one, or any ſigns of lava or pumice-ſtone, directly conclude ſuch a mountain not to be volcanic: whereas, only ſuppoſe Mount Etna to have ceaſed erupting for many ages, and that half of its conical part ſhould have mouldered away by time (which would naturally be the conſequence) and the harder parts remain in points, forming an immenſe circuit of mountains (Etna extending at its baſis more than one hundred and fifty miles); ſuch an obſerver as I have juſt mentioned would certainly not find a crater on the top of any of theſe mountains, and his ideas would be too limited to conceive, that this whole range of mountains were only part of what once conſtituted a complete cone and crater of a volcano. It cannot be too ſtrongly recommended to obſervers in this, as well as in every other branch of natural hiſtory, not to be over-haſty in their deciſions, nor to attribute every production they meet with to a ſingle operation of nature, when perhaps it has undergone various, of which I have given examples in the iſland which has been the principal ſubject of this letter. That which was one day in a calcareous ſtate, and formed by an inſect in the ſea, becomes vitrified in another, by the action of the volcanic fire, and the addition of ſome natural ingredients, ſuch as ſea ſalts and weeds, and is again transformed to a pure clay by another curious proceſs of nature. The naturaliſt may indeed decide as to the preſent quality of any natural production; but it would be preſumption in him to decide as to its former ſtates. As far as I can judge in this curious country, active nature ſeems to be conſtantly employed in compoſing, decompoſing, and recompoſing; but ſurely for all-wiſe and benevolent purpoſes, though on a ſcale perhaps much too great and extenſive for our weak and limited comprehenſion.

I have the honour to be, with great regard and eſteem, &c.

W. HAMILTON.

POSTSCRIPT.

THE earth is not yet ſo perfectly quiet in Calabria and at Meſſina, as to encourage the inhabitants to begin to rebuild their houſes, and they continue to live in wooden barracks. There has, however, been no earthquake of conſequence during theſe laſt three months. My conjecture, that the volcanic matter (which was ſuppoſed to have occaſioned the late earthquakes) had vented itſelf at the bottom of the ſea between Calabria and Sicily, ſeems to have been verified; for the pilot of one of his Sicilian Majeſty's ſciabecques, having ſome time after the earthquakes caſt anchor off the point of Palizzi, where he had often anchored in twenty-five fathom water, found no bottom till he came to ſixty-five fathom, and having ſounded for two miles out at ſea towards the point of Spartivento in Calabria, he ſtill found the ſame conſiderable alteration in the depth of the ſea. The inhabitants of Palizzi likewiſe declare, that during the great earthquake of the 5th of February, 1783, the ſea had frothed and boiled up tremendouſly off their point.


ISOLA
Porto

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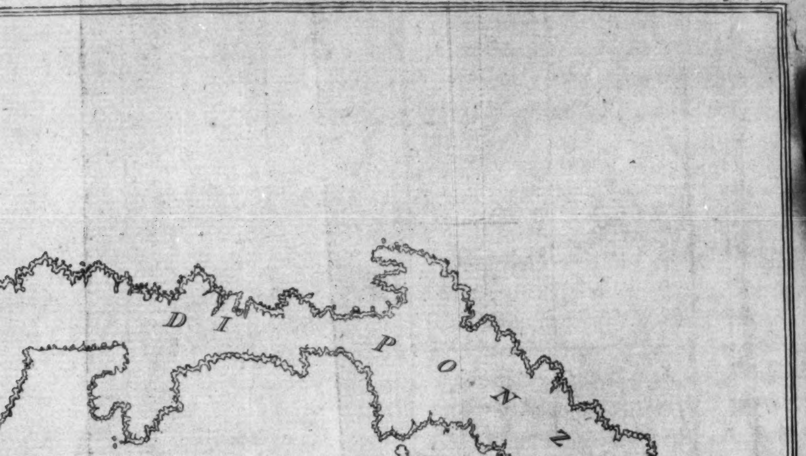

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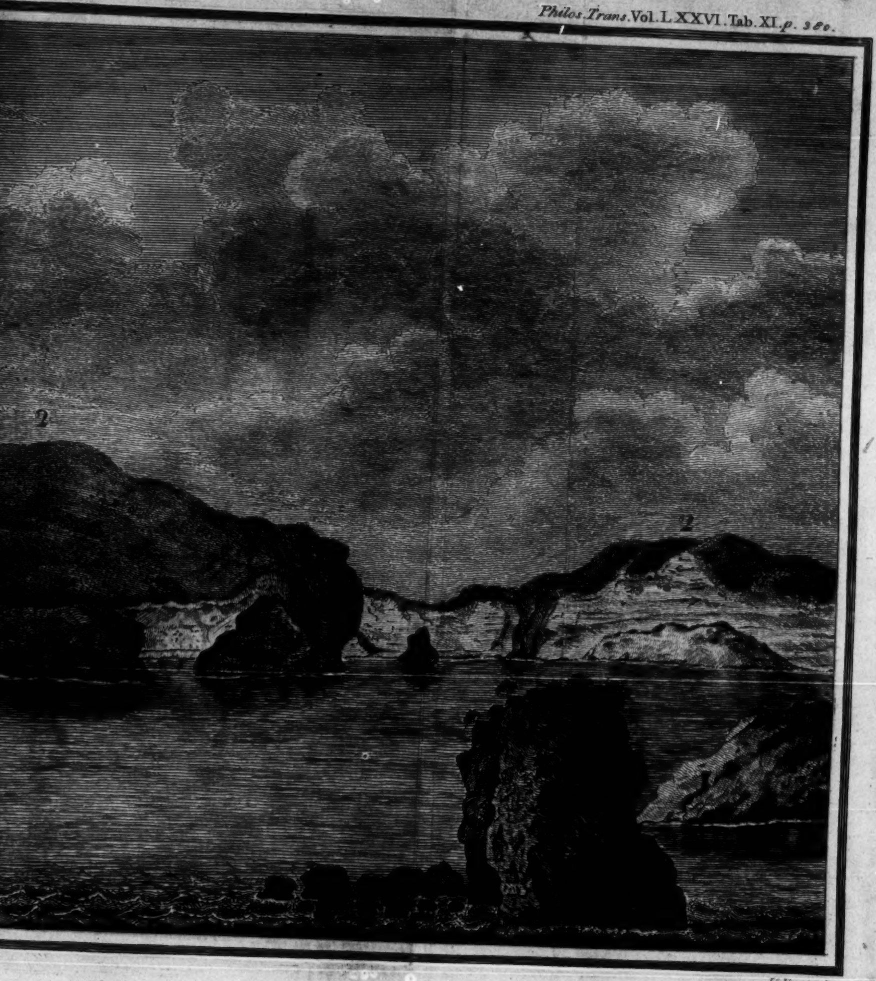

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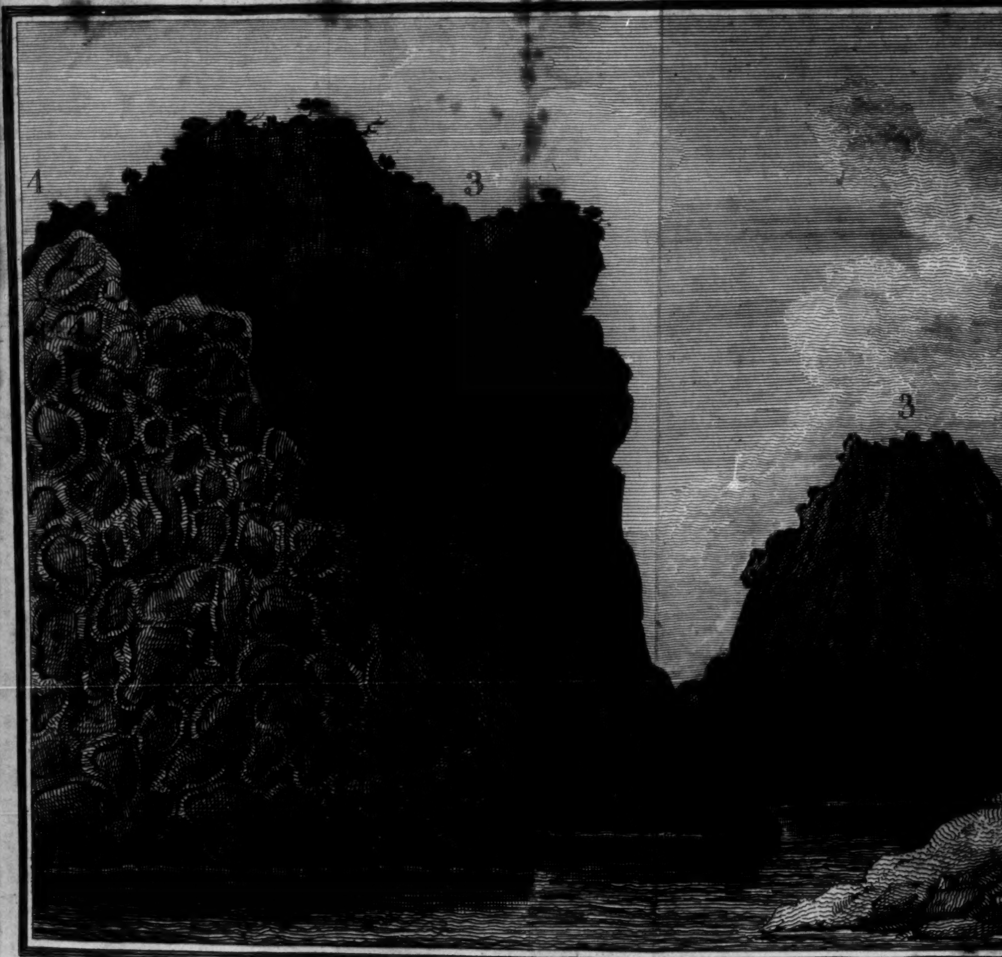

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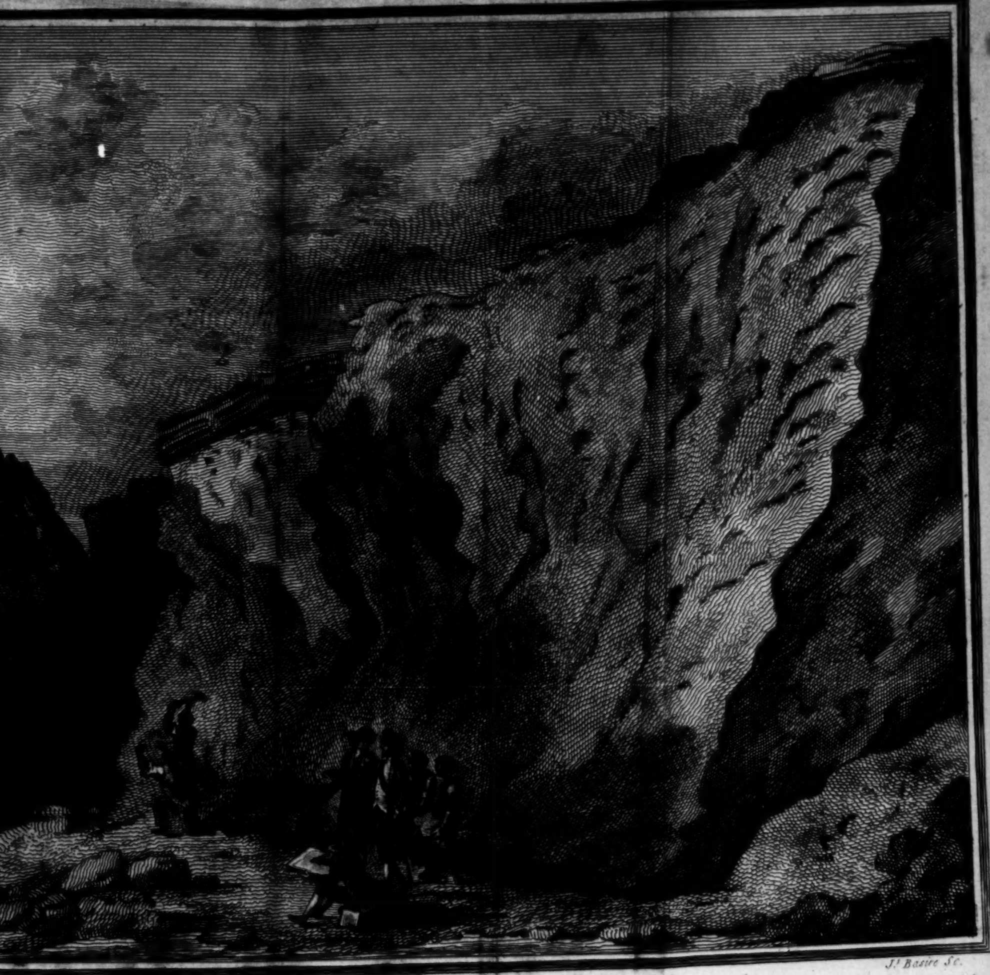

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EXPLANATION OF THE PLATES.

Tab. X. Plan of the iſland of Ponza.

Tab. XI. View of part of the inſide of the harbour of the iſland of Ponza.

Fig. 1. Rock of lava, which in many parts is formed into regular ſmall baſaltes of a reddiſh caſt, having probably been tinged with ſome ochre. Moſt of the detached rocks of this iſland reſemble this.

Fig. 2. See p. 374.

Tab. XII. View taken from the outſide of the harbour of the iſland of Ponza, near the Lighthouſe.

Fig. 1. Rock of volcanic matter converted to pure clay.

— 2. Ditto, with ſtrata of pumice-ſtone.

— 3. Rocks of lava inclining to take baſaltic forms.

— 4. Rock compoſed of ſpherical baſaltes.

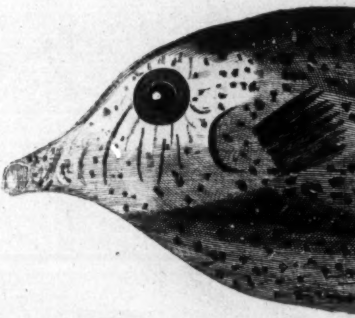
XIX. *An Account of a new Electrical Fiſh. In a Letter from Lieutenant* William Paterſon *to Sir* Joſeph Banks, *Bart. P. R. S.*

Read May 11, 1786.

SIR,

WHILE at the iſland of Johanna, one of the Comora iſlands, in my way to the Eaſt-Indies, with the 98th Regiment, I met with an electrical fiſh, which has hitherto eſcaped the obſervation of naturaliſts, and ſeems in many reſpects to differ from the electrical fiſhes already deſcribed; which induces me to ſend you the following account of it, with a very imperfect Drawing, and to beg that, if you think it deſerves attention, you will do me the honour of preſenting it to the Royal Society. The ſituation of a ſubaltern officer, in an army upon foreign ſervice, will, I hope, ſufficiently apologize for my ſending you ſo very imperfect a ſketch of the fiſh, which was made in the field, in a hot climate, under every diſadvantage.

The fiſh is ſeven inches long, two inches and a half broad, has a long projecting mouth, and ſeems to be of the genus Tetrodon. The back of the fiſh is a dark brown colour, the belly part of ſea-green, the ſides yellow, and the fins and tail of a ſandy green. The body is interſperſed with red, green, and white ſpots, the white ones particularly bright; the eyes large, the iris red, its outer edge tinged with yellow. (See Tab. XIII.)



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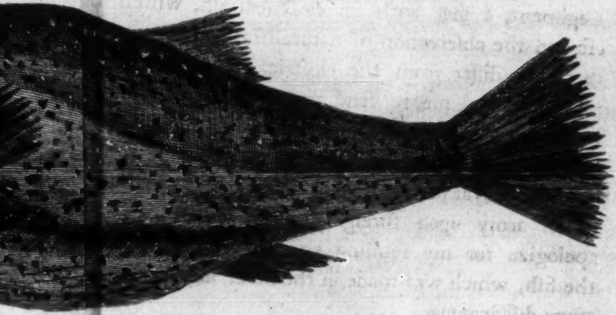

Basire Sc.

The iſland of Johanna is ſituated in latitude 12° 13′ ſouth. The coaſt is wholly compoſed of coral rocks, which are in many places hollowed by the ſea. In theſe cavities I found ſeveral of the electrical fiſhes. The water is about 56° or 60° of heat of FAHRENHEIT's thermometer. I caught two of them in a linen bag, cloſed up at one end, and open at the other. In attempting to take one of them in my hand, it gave me ſo ſevere an electrical ſhock, that I was obliged to quit my hold. I however ſecured them both in the linen bag, and carried them to the camp, which was about two miles diſtant. Upon my arrival there, one of them was found to be dead, and the other in a very weak ſtate, which made me anxious to prove, by the evidence of others, that it poſſeſſed the powers of electricity, while it was yet alive. I had it put into a tub of water, and deſired the Surgeon of the regiment to lay hold of it between his hands; upon doing which he received an evident electrical ſtroke. Afterwards the Adjutant touched it with his finger upon the back, and felt a very ſlight ſhock, but ſufficiently ſtrong to aſcertain the fact.

After ſo very imperfect an account, I will not trouble you with any obſervations of my own upon this ſingular fiſh; but beg you will conſider this only as a direction to others who may hereafter viſit that iſland, and from their ſituation, and knowledge in natural hiſtory, may be better able to deſcribe the fiſh, and give an account of its electrical organs.

I have the honour to be, with great eſteem, &c.

W. PATERSON,
Lieutenant 98th regiment.

* * *

XX. *Obſervation of the Tranſit of Mercury over the Sun's Diſc, made at* Louvain, *in the* Netherlands, *May 3, 1786. By* Nathaniel Pigott, *Eſq. F. R. S.*

Read June 15, 1786.

Louvain, May 15, 1786.

THE tranſit of Mercury was to happen a few days after my arrival at this place from England. Although I brought no aſtronomical inſtruments with me, I wiſhed to obſerve this phænomenon; and upon application to M. THYSBAERT, *Préſident du Collège Royal*, a very diſtinguiſhed Member of this Univerſity, he ſupplied me, in the politeſt manner, with the following inſtruments, and a convenient place for the obſervation. He carried his attention to the moſt trifling circumſtances, in order to make my ſituation, in every reſpect, agreeable. The inſtruments he provided me with were a Gregorian reflector of 21 inches focal length, with an aperture of 4½ inches, the magnifying power of which I eſteemed about 70 or 80, with a good quadrant 18 inches radius, and a compound pendulum clock, ſteadily fixed, beating dead ſeconds. Theſe inſtruments were made in London, and uſed for the obſervation of Mercury. The rate of the clock, and the apparent times thence deduced, were obtained by equal altitudes of the ſun, taken with the quadrant. Theſe were the only inſtruments I had, and therefore ſuch obſervations as are not dependent on the *meaſure of time*, are to be conſidered as made by eſtimation; however, the moſt important, the internal and external contacts of Mercury, and hence the egreſs of his center and the interval of time between the two contacts, were made in a very ſatisfactory manner. About ſix o'clock, when I attended for the obſervation, there being a great number of ſolar ſpots, Mercury might eaſily have been miſtaken for one; but his motion ſoon removed every doubt in that reſpect. Flying clouds obſcured the ſun at intervals; but during the laſt half hour, the weather was fine, the ſky clear, the limb of the ſun well defined; Mercury round and very black. There ſeems to have been ſome miſtake, in reſpect of this phænomenon, either in the calculation or the printing of the *Connoiſſance des Temps* of this year: the emerſion of the center of Mercury is there ſet down at 19 h. 45′ apparent time at Paris; whereas, by my obſervation, the egreſs of the center at Louvain was at 20 h. 47′ 28″ or 29″ apparent time. Taking here no other equation into conſideration, except the difference of meridians between Paris and Louvain, which, by a great number of obſervations, I determined in 1775 to be 9′ 37″ in time*, the emerſion of the center at *Paris* muſt have been at 20 h. 37′ 51″ or 52″, which differs nearly 53′ from the computed time. By the ſame reaſoning, I ſhould ſuppoſe, that the emerſion of the center of Mercury at Greenwich was obſerved at 20 h. 28′ 35″ or 36″. Mercury being ſo very near the earth, the effects of parallax muſt be conſiderable, and the weſtern ſituation of Greenwich of 18′ 53″ in time from this place, muſt occaſion a retardation, which, on computation, may be hereafter allowed for, and added to the ſuppoſed time of the egreſs above-mentioned, deduced from my obſervation here.

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* See Philoſophical Tranſactions, vol. LXVIII. p. 654.

While I am writing this Paper, the reſpective ſituation of Greenwich and Louvain ſtrikes me. The latitude of Greenwich is 51° 28′ 40″, that of Louvain 50° 53′ 3″ *; the difference little more than half a degree. Greenwich is 9′ 16″ weſt, and Louvain 9′ 37″ eaſt of the Paris Obſervatory; the parallax above-mentioned is therefore nearly, but in a contrary ſenſe, equal at the two places, and thus the effects of both are compenſated relatively to Paris. What other advantage may reſult from this circumſtance, would require conſideration. I have not leiſure, at preſent, to revolve it in my mind, as I am deſirous to lay this Paper before the Royal Society as ſoon as I can, by the favour of Dr. MASKELYNE, our Aſtronomer Royal.

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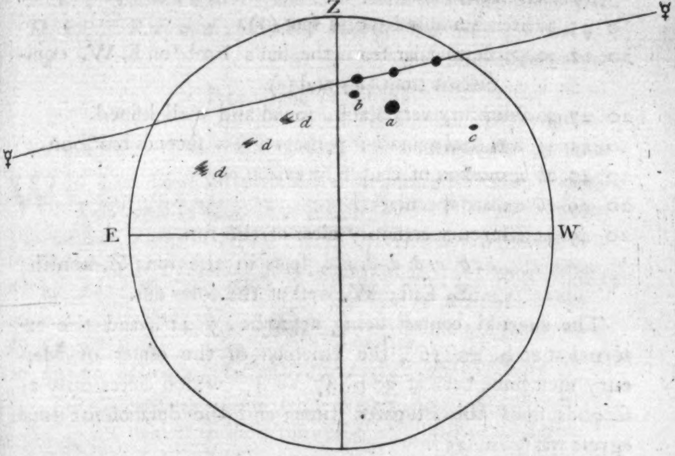
* See Philoſophical Tranſactions, vol. LXVIII. p. 643.

Obſervations of the Tranſit of Mercury at Louvain.



Apparent time.

H.	M.	S.	
18	32	30	flying clouds; Mercury ill defined, with ſome twiſting.
19	13	30	the ſpot (*a*) appears thrice as large as Mercury; ſpot (*b*) twice ditto.
19	16	30	a perpendicular from the ſun's limb on E. W. biſects Mercury and (*b*).
19	27	30	perpendicular, as above, equi-diſtant from (*a*) and (*b*).

Apparent time.			
H.	M.	S.	
19	34	30	perpendicular, as above, biſects Mercury and ſpot (*a*).
19	42	30	perpendicular from Mercury on E. W. is beyond ſpot (*a*).
19	45	30	it is ſenſibly beyond ſpot (*a*).
20	12	30	perpendicular from the ſun's limb on E. W. equidiſtant from (*a*) and (*c*).
20	27	30	Mercury very black, round and well defined.
20	45	41	internal contact; perhaps a few ſeconds too ſoon.
20	47	26	emerſion of center by *eſtimation*.
20	49	16	external contact.
20	49	41	Mercury certainly clear of the ſun.

a. b. c. d. d. d. are ſpots in the ſun; Z. zenith; E. Eaſt; W. weſt of the ſolar diſc.

The internal contact being at 20 h. 45′ 41″, and the external at 20 h. 49′ 16″, the emerſion of the center of Mercury muſt have been at 20 h. 47′ 28″½; which differs only 2½ ſeconds from the eſtimated time; and the duration of total egreſs was 3 m. 35 ſ.

N. B. The reaſons why the nine firſt obſervations are all marked at 30″ is, that in reality they were ſet down at the *minute* only; and that I have added 2′ 30″ to each to reduce the time by the clock to apparent time; more nicety would have been ſuperfluous: but the *four laſt* were rigorouſly computed.

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XXI. *Obſervation of the late Tranſit of Mercury over the Sun, obſerved by* Edward Pigott, *Eſq. at* Louvain *in the* Netherlands; *communicated by him in a Letter to* Nevil Maſkelyne, *D. D. F. R. S. and Aſtronomer Royal.*

Read June 15, 1786.

SIR, Louvain, May 27, 1786.

WE have been fortunate here in ſeeing Mercury's egreſs. I obſerved it thus:

Apparent time, May 3.

H.	M.	S.	
20	45	25	Mercury's limb in contact with the ſun's limb; uncertain.
20	45	37	ditto ditto; certain.
20	47	17	Mercury biſected by the ſun's limb.
20	49	22	Mercury quite out; clouds for a ſhort interval, which renders the obſervation rather doubtful.

Though the air was not perfectly free from thin clouds, nevertheleſs the limbs were well defined. At 20 h. 45′ 25″, when I firſt judged Mercury's limb in contact with the ſun's, his form, I think, became rather oval. Theſe obſervations were made with RAMSDEN's two-feet achromatic, magnifying about 70 times. The above times diſagreeing ſo conſiderably with the tables will, I imagine, not a little ſurpriſe M. DE LA LANDE.

I remain, Sir, with great regard, &c.

EDW. PIGOTT.

XXII. *Additional Obſervations on making a Thermometer for meaſuring the higher Degrees of Heat.* *By* Mr. Joſiah Wedgwood, *F. R. S. and Potter to Her Majeſty.*

Read June 22, 1786.

MY thermometer for meaſuring the higher degrees of heat having been honoured with the notice of this illuſtrious Society, I now requeſt a further indulgence for a few more obſervations on the ſame ſubject.

In my firſt Paper * I communicated every thing that experience had then taught me, reſpecting both the conſtruction and uſe of this thermometer; but more extenſive practice has ſince convinced me, that other managements and precautions are neceſſary, in order to bring it to the perfection it is capable of receiving: for pieces made of the ſame clay, and exactly of the ſame dimenſions, have been found to differ in the degree of their diminution by fire, in conſequence of ſome circumſtances in the mode of their formation, at that time unheeded, and very difficult to be developed.

Of the two ways propoſed for forming them, the mould and the preſs, the former was made choice of, as being, for general uſe, the moſt commodious. The ſoft clay was preſſed into a ſquare mould with the fingers; and the pieces, when dry, were pared down on two oppoſite ſides, by means of a paring gage made for that purpoſe, ſo as to paſs exactly to 0° at the entrance of the converging canal of the meaſuring gage.

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* Philoſophical Tranſactions, Vol. LXXII.

But the pieces thus formed have been found liable, in paſſing through ſtrong fire, to receive a little alteration in their figure, which produces an uncertainty with reſpect to their ſubſequent meaſurement: the two ſides, inſtead of continuing flat, become concave; the edges, both at top and bottom, projecting beyond the middle part, ſometimes very conſiderably, as at *a* and *b*, fig. 1, (Tab. XIV.) where AB repreſents a perpendicular ſection of an unburnt piece, and *a b* a like ſection of the ſame piece after it has undergone a heat of 160 degrees. This irregularity in the form, which is ſenſible only after paſſing through the high degrees of fire, was obſerved in ſome of the early experiments, but was not then looked upon as being productive of any error.

On more attentively examining this matter, it appeared, that when the clay is preſſed into a mould, the ſurface in contact with the mould acquires a more compact texture than the inner part of the maſs;—that this compactneſs reſtrains, in ſome degree, its diminution in the fire;—and therefore, that when this ſurface, or leſs diminiſhable cruſt, is pared off from the two ſides only, the piece may be conſidered as having its upper and lower ſtrata (AA and BB, fig. 1.) compoſed of a leſs diminiſhable matter than the intermediate part, the neceſſary conſequence of which ſtructure will be ſuch a figure as we find the pieces to aſſume; for if any ſtratum in the maſs ſhrinks leſs than the reſt, the extremities of that ſtratum muſt be left proportionably prominent. That this was the true cauſe of the inequality, I was convinced by firing ſome pieces *unadjuſted*, with all their ſurfaces entire, as they came from the mould; for theſe pieces, after paſſing through the ſame ſtrong fires with the preceding, continued flat, with the angles regularly ſharp, and without the leaſt ſenſible prominence in any part.

Some of the moulds, employed for this uſe, were made of plaſter, a material more convenient for the workman than metal, as the pieces part more freely from it, but which contributed greatly to increaſe the above-mentioned irregularity: for the plaſter, by abſorbing a portion of the water from the clay contiguous to it, renders the ſurface at the ſame time, even at the inſtant of contact, much more *conſiſtent*, and conſequently more difficult to preſs into the angles of the mould; ſo that the outſides of theſe pieces were not only more *compreſſed*, but formed of clay of a different *temper* from the inner parts, being much drier or firmer; a circumſtance which, as will appear hereafter, reſtrains ſtill more their diminution in the fire.

The moulds were therefore laid aſide, and the preſs adopted in their ſtead; for as the ſoft clay, preſſed in a cylindrical veſſel, gives way and eſcapes through an aperture made for that purpoſe (by which means it is formed into long rods), the ſides of the piece cannot be ſuppoſed to receive ſo great a degree of compreſſure againſt the ſides of the aperture through which it is *delivered* in this operation, as it does againſt the ſides of the mould, by which it is *confined* till every part has born a preſſure ſufficient to force the clay into every angle, which is much greater than even a workman would imagine till he comes to try the experiment himſelf.

But with this change ſome new difficulties aroſe; for pieces preſſed through the ſame aperture, and from the ſame preſſ-ful of clay, and adjuſted, when dry, to the ſame point in the gage, were found, after paſſing together through the ſame ſtrong fires, to differ in their dimenſions from one another, in ſome inſtances more than any of the preceding.

Having hitherto paid no particular attention myſelf to the mere manual labour of preſſing the clay, I determined, upon this event, to go through that and every other operation, however ſimple and ſeemingly inſignificant, with my own hands. In doing this I obſerved, that the power neceſſary for forcing the clay through an aperture which bore but a ſmall proportion to the diameter of the maſs of clay in the preſs, was ſo great as to ſqueeze out, along with the clay that firſt paſſed through, a conſiderable portion of the water that belonged to the reſt. From this over-proportion of water in the compoſition of the firſt pieces they were ſoft and ſpongy, and the ſucceeding ones more and more compact, till at length the clay proved ſo ſtiff as ſcarcely to be forced through at all.

Clay, containing different proportions of water, is well known to diminiſh differently in drying; but it was not imagined that, when dry, there would be any difference in its ſubſequent diminutions by fire. Experiments however, multiplied in a variety of circumſtances, ſhewed deciſively, what the pieces formed in the mould had given grounds to ſuſpect, that thoſe formed of the ſofteſt clay, and which had undergone the leaſt preſſure, diminiſhed moſt in burning; and that the diminution is uniformly leſs and leſs, in proportion to the greater degree of preſſure or compactneſs.

The knowledge of the cauſe of the irregularity ſuggeſted a remedy. I leſſened the width of the preſs very much, ſo as to bring the diameter of the maſs of clay, and that of the aperture through which it is delivered, to a nearer proportion with one another. A much leſs degree of force being now ſufficient, the pieces, or rods, were proportionably more uniform, though there was ſtill a ſenſible difference, in conſiſtence, between thoſe which were firſt and laſt preſſed out from the ſame maſs of clay. The intermediate ones, within a certain diſtance from the two extremes, correſponded very nearly with one another; ſo that by rejecting a ſufficient number of the firſt and laſt, and uſing the *intermediate ones only*, the inequality may be conſidered as almoſt annihilated.

I nevertheleſs ſtill found that, in ſtrong fire, the edges became a little prominent, though not ſo much as before. I was aware that theſe pieces muſt partake, in ſome degree, of the imperfection of thoſe made in the mould; having their ſurfaces rendered, by their friction againſt the ſides of the aperture, more compact than the inner part. But I ſuſpected that ſomething might depend alſo upon the *form*, and accordingly made many trials for aſcertaining the form that might be leaſt liable to this irregularity: the angles only were bevilled off, the ſides were rounded, the pieces were rounded all over, made of ovals and other curves, and both the longeſt and ſhorteſt dimenſions were uſed as the extent to be meaſured: the general reſult was, that the nearer they came to a circular figure, the leſs inequality they contracted in the fire, and by making them entirely circular, the imperfection appeared to be obviated altogether; cylindric pieces bearing the ſtrongeſt fires without the leaſt appearance of prominence or inequality in any part of their ſurface. I have therefore choſen this laſt form, leaving only one narrow flat ſide (*a b*, fig. 2.) as a bottom for the pieces to reſt upon, and to diſtinguiſh the poſition in which they are to be meaſured in the gage.

I have endeavoured at the ſame time to obviate whatever inaccuracy the inequality of compactneſs may be capable of producing, by ſo adjuſting the aperture through which the rods are preſſed, and on which their figure and dimenſions depend, as to ſuperſede the uſe of the paring gage altogether; that the whole ſurface may remain of the ſame uniform compactneſs which it received in the preſs. And as it is ſcarcely practicable, in any mode of forming ſoft clay, to have all the pieces preciſely of the ſame dimenſions after drying, I do not reject thoſe which come within two or three degrees of the ſtandard, but, inſtead of injuring the ſurface by paring or rubbing, I mark on the ends the degrees which they reſpectively exceed or fall ſhort; which degrees are accordingly to be ſubſtracted, or added, in all obſervations of heat made with thoſe pieces. Strictly ſpeaking, an allowance ought to be made alſo for the proportional diminution upon this exceſs or deficiency itſelf; but the allowance for three degrees would not, at the melting heats of copper, ſilver, or gold, amount to more than a ſeventh part of a degree; and at the extreme point of heat that I have been able to attain, when the piece has diminiſhed $\frac{160}{600}$, or nearly one-fourth of its whole thickneſs, it comes only to four-fifths of a degree.

It may be proper to take notice of an irregularity in the *apparent* diminutions of the pieces, which was ſometimes obſerved to happen from another cauſe, their bending a little in ſtrong fire, ſo as to be prevented from going ſo far in the gage as they would have done if they had continued perfectly ſtraight. But as this takes place only in pieces of conſiderable length, and as they derive no advantage of any kind from that length, the remedy is too obvious to need being here mentioned.

Another fallacious appearance aroſe, not from any imperfection in the pieces themſelves, but from a deception with reſpect to the heat in which the compariſon of them had been made. In one period of the courſe of my experiments, I employed, for firing them, a ſmall, ſhallow, cylindrical veſſel (fig. 3.) ſetting the pieces on end, cloſe together, on its bottom, and placing it in the middle of the fuel, in a common air-furnace, with care to keep the fire as equal all round it as poſſible. It was expected, that all the pieces would receive an equal heat; and as they were found, after the operation, to differ in their dimenſions, ſometimes conſiderably, from one another, theſe differences proved a ſource of much perplexity, till it was diſcovered that the pieces had really undergone unequal degrees of heat.

In the paper on the compariſon of this thermometer with Fahrenheit's*, I have taken notice of the great difficulty of obtaining, in ſmall furnaces, a perfectly equal heat, even through the extent occupied by a few of theſe little pieces; and how different the heat may be in different parts of one veſſel, we may be ſatisfied by an eaſy experiment, *viz.* ſetting a cylindrical rod of clay, of the length of eight or ten inches, upright in the middle of a crucible, and urging it with ſtrong fire in a common ſmall furnace; the rod will be found very differently diminiſhed at different parts of its height; and if its height be ſufficient to reach ſome way above the fuel, nearly the whole range of the thermometric ſcale may be produced in one rod; an ocular proof, not only of the diverſity of heat within a ſmall compaſs, but likewiſe of the *peculiar* ſenſibility of this thermometer, every *part* of the maſs expreſſing diſtinctly the degree of heat which it has itſelf felt. It will be proper, in this experiment, to have a tube fixed in the bottom of the crucible, for keeping the rod ſteady, as at fig. 4. By this means the heat of my air-furnace renders a rod of the thermometric clay tapering, from about four parts in diameter at top to three at bottom, which are nearly the proportions between the width of the piece when unburnt, or but juſt ignited, and when it has ſuffered a heat of 160 degrees.

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* Philoſophical Tranſactions, vol. LXXIV.

To the foregoing ſources of inequality in the pieces, one more may be added, ſmall cavities, or air-bubbles accidentally incloſed, which ſometimes happened in the earlier experiments. In order to prevent theſe, particular attention is now paid by the workmen to what we call *banding* or *ſlapping* the clay, an operation by which its different parts are intermixed, and the maſs rendered of an uniform temper throughout. The workman takes a lump of the clay in his hands, perhaps of two pounds weight, and, breaking it in two in the middle, lays one part upon the other, and preſſes them flat again, repeating this forty or fifty times, or perhaps oftener. Now, conſidering the pieces at firſt as two diſſimilar maſſes, with any number of air-bubbles incloſed; each of theſe pieces being by the firſt doubling divided into two, by the next into four, by the third into eight, and ſo on in geometrical progreſſion, each of the original maſſes will be divided by the fiftieth repetition into upwards of eleven thouſand millions of millions of inviſible laminæ:—inviſible, becauſe the lump of clay would, long before the laſt doubling, be of one uniform colour, though at firſt one-half of it had been black, and the other white. If therefore no air be incloſed between the pieces at the times of their being put together in this proceſs, all the air which might have been in the maſs before would certainly be driven out; and, to avoid as much as poſſible the introduction of any freſh portions of air, the two ſeparated pieces are each time made ſmooth, and a little convex, on the ſurfaces that are to be brought together.

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By due attention to the circumſtances above ſtated, any ſingle quantity of clay may be made up into thermometer-pieces, that ſhall differ very little, if any thing at all, from one another.

But a new difficulty now aroſe, more embarraſſing than any of the former; that of procuring freſh ſupplies of clay, of the ſame thermometric quality with the firſt. The quantity of the clay which, after trial of many others, I had made choice of, was ſmall; but the particular ſpot it was taken from being known, and having purchaſed the little eſtate in which it was raiſed, I had not a doubt of obtaining more of the ſame when it ſhould be wanted: for clays in general, when raiſed from an equal depth, in the ſame ſtratum, and near the ſame place, are found to poſſeſs the ſame properties, with reſpect to ductility in the hands of the workman, a diſpoſition to aſſume by fire a porcelain or vitreous texture, ſingly or in compoſition, and all other qualities relative to their uſe in pottery. In this, however, I was deceived; for when a freſh ſupply was wanted, to complete my experiments, though I had ſome taken from a pit joining to the firſt, and at the ſame depth, it was found to diminiſh differently from the former parcel. I then had ſome raiſed from different parts of the ſame field and bed, and at different depths; and in various other places in Cornwall, from the ſpot where this ſpecies of clay is firſt met with to the Land's-End; but all theſe clays differed ſo much from the firſt in the quantity of their diminution by fire, and moſt of them likewiſe from each other, that I deſpaired of being ever able to find one that would correſpond with it, or any natural clays that could be obtained twice of exactly the ſame *thermometric* properties, how ſimilar ſoever in other reſpects.

Upon a review of the numerous compariſons which I have made of theſe new clays, in different degrees of heat, from the commencement of redneſs up to intenſe fire, the moſt ſtriking differences of the greateſt part of them from the old ſeemed to originate in the lower ſtages of heat; and of thoſe which were got from the neighbourhood of the old, the variations from it in the higher ſtages ſeemed, for the moſt part, to be only conſequences of thoſe differences in the lower ones.

I have mentioned, in the firſt Paper, that the original thermometer-pieces had their bulk enlarged a little on the approach of ignition; but that by the time they became viſibly red-hot throughout, they are reduced to their former dimenſions again; and at this moment the thermometric diminution begins. The new clays had their bulk enlarged in a much greater proportion, and the enlargement was of much longer continuance: ſome of them required a heat of 15 degrees to deſtroy the increaſe which ignition had produced in their bulk, and bring them back to their original dimenſions: after this period, moſt of them diminiſhed pretty regularly, and uniformly with the old, being nearly ſo many degrees behind it, in all the ſucceeding ſtages of heat, as they required to bring them back from the enlarged ſtate.

I have mentioned alſo, in my former paper, that a quantity of air is extricated from the clay, moſt rapidly at the period in which the augmentation of bulk takes place; and that the augmentation was probably owing to this air forcing the particles of the clay a little aſunder, previous to the inſtant of its eſcape. It was therefore preſumed, that the greater extenſion of theſe new clays might be owing, either to a greater quantity, or ſtronger adheſion, of this combined air: and as clay, kept moiſt for a length of time, in certain circumſtances, undergoes a proceſs ſeemingly analogous to fermentation,

tion, it was hoped that, by ſuch a proceſs, part of its combined air might be detached.

But experiments made on this idea have proved, that theſe clays, kept moiſt for a twelvemonth,—kept for a conſiderable length of time in a heat juſt below viſible redneſs,—boiled in water for many hours,—alternately, and repeatedly, moiſtened and dried,—ſuffer no alteration in their thermometric properties, and continue to differ from the ſtandard clay juſt as much as they did at firſt.

Some of theſe new clays differed from the old in a property ſtill more eſſential, and by which I was much more diſconcerted; for though they continued diminiſhing with tolerable regularity, keeping only ſome degrees behind it, up to a certain period of heat, about that in which caſt iron melts; yet many of the pieces, urged with a heat known to be greater than that, were found not to be diminiſhed ſo much as thoſe which had ſuffered only that lower heat. Further experiments ſhewed, that, after diminiſhing to a certain point, they begin, upon an increaſe of the heat beyond that point, to ſwell again: and as this effect is conſtant in certain clays, and begins earlieſt in thoſe which are moſt vitreſcible, and as clays are found to ſwell upon the approach of vitrification, I look upon this ſecond enlargement of bulk, however inconſiderable, as a ſure indication of the clay or compoſition having gone beyond the true porcelain ſtate, and of a diſpoſition taking place towards vitrification; which ſtage is always, ſo far as my experience reaches, attended with a new extrication of air; and in ſome inſtances, this air being unable to make its eſcape from the tenacious maſs that envelopes it, the burnt clay is thereby ſo much increaſed in bulk as to ſwim on water like very light wood. The degree of heat therefore, at which this enlargement begins, may be conſidered as a criterion of the degree of vitreſcibility of the compoſition; which points out a new uſe of this thermometer, enabling us to aſcertain the *degree of vitreſcibility* of bodies that cannot actually be vitrified by any fires which our furnaces are capable of producing.

All my reſearches among the natural clays proving fruitleſs, and the experiments having ſhewn that all thoſe, which could ſufficiently reſiſt vitrification, diminiſhed *too little* in the fire, I endeavoured to find a body poſſeſſed of the oppoſite property, that is, diminiſhing *too much*, and, by a mixture of theſe two, to produce the *medium* diminution required. As I could not find any natural ſubſtance poſſeſſed of that property, which would not at the ſame time render the compound too vitreſcible, I was obliged to have recourſe to ſome artificial preparation; and as the earth of alum is the pure argillaceous earth, to which all clays owe their property of diminution in the fire, poſſeſſing that property in a greater or leſs degree according to the quantity of alum earth in their compoſition, I mixed ſome of this earth with the clay, and found it to anſwer my wiſhes completely, both in procuring the neceſſary degree of diminution, and increaſing its unvitreſcibility. So little is this compound diſpoſed to vitrification, that the greateſt heat I could give it, that of 160°, did not even bring it to a porcelain texture, but left it ſtill bibulous; and as it does not arrive at the *porcelain* ſtate in this fire, there can be no danger of its approaching too near to the *vitreſcent* in any heat that we can produce in a furnace.

In order to obtain the exact medium required, I took one of the beſt of the clays I had procured from Cornwall, and mixed it with different proportions of the alum earth, till the compoſition was found, on repeated trials, to agree with the original in all degrees of heat. This coincidence was not indeed eſſential; but as many degrees of heat were already before the public, meaſured by thermometer-pieces made of the firſt clay, and as the correſpondence of the firſt with FAHRENHEIT's ſcale had likewiſe been in ſome meaſure aſcertained, it was deſirable that the ſame degrees of heat ſhould continue to be expreſſed by the ſame numbers.

The alum earth is prepared for this purpoſe by diſſolving the alum in water, precipitating with a ſolution of fixed alkali, and waſhing the earth repeatedly with large quantities of boiling water: when the earth has ſettled, the water above it is let off by cocks in the ſides of the hogſheads; and when the veſſels are filled up with freſh water, care is taken to ſtir up the earth from the bottom, and mix it thoroughly with the liquor. I find it moſt convenient to uſe the earth undried, in its gelatinous ſtate, as in this ſtate it unites eaſily and perfectly with the clay; whereas, when the alum earth has concreted into dry maſſes, great labour is neceſſary to mix them uniformly together.

I have tried ſeveral different parcels of Engliſh alum, from the ſame and from different manufactories, and found no material difference in the quantity of earth it contains*. Nor indeed would it be of any conſeqence if there was a difference in this reſpect, as the proportion of alum earth neceſſary for different clays, and even for different parcels of the ſame clay, can only be aſcertained by repeated trials, adding ſucceſſive quantities of the earth till the deſired effect is found to be produced. Ten hundred weight of the Cornwall porcelain clay which I have now in uſe required all the earth that was afforded by five hundred weight of alum.

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* A difference in the quantity of earth *may* ariſe from different proportions of GLAUBER's ſalt and vitriolated tartar, of which I have found quantities very conſiderable, but nearly alike, in all the Engliſh alum I have examined. Theſe ſalts are doubtleſs formed by the kelp aſhes employed in the preparation of the alum. They are diſcovered by calcining the dried alum with charcoal powder, which decompoſes the alum only, leaving the other two ſalts intermixed with the alum earth, from whence they may be extracted by water.

It is material in this place to obſerve, that the earth of alum is extremely tenacious of water, inſomuch that, though apparently dry, the water and air amount to near as much as the pure earth, and are not to be completely driven out without a full red heat. When divided by the admixture of other earthy bodies, it parts with its water eaſier indeed than before; but a mixture containing ſo much of it as the thermometric compoſition does, is far more retentive of water than common clay, and requires to be kept for ſome time in a heat equal to that of boiling water, before it is to be conſidered as dry, that is, before the adjuſtment of the pieces in the gage. If they are adjuſted when only apparently dry, or of ſuch a degree of dryneſs as they can be brought to by a heat that the hand can bear, the heat of boiling water will diminiſh them two or three degrees; and the greateſt part of what they have thus been deprived of, they gradually recover again on being expoſed to the atmoſphere, ſo that the adjuſtment muſt be made immediately after the boiling heat.

By the ſame expedient to which I have thus been obliged to have recourſe for procuring to the porcelain clay of Cornwall the ſtandard degree of diminution, and reſiſtence to fire, the ſame qualities may probably be communicated to any other clay that is tolerably pure from calcareous earth and iron; ſo that the thermometer clay is no longer to be conſidered as the produce of any particular ſpot (which was the principal inconvenience originally imagined to attend it), but may be procured and prepared in all parts of the world where good common clay, and alum, are to be found; and *correſponding* thermometers may, conſequently, be conſtructed, without any ſtandard to copy from. For, if a converging canal be formed, of any convenient length, with the widths at the two ends in the proportion of 5 to 3, with the ſides perfectly ſtraight, and divided into 240 equal parts, numbering the diviſions from the wider end*;—and if a clay be obtained of ſuch quality, that when formed, in the manner already mentioned, into pieces of ſuch ſize as to enter to 0 in the gage or canal, theſe pieces ſhall juſt begin to diminiſh, or go a little further in the canal, by a heat viſibly red;—go to 27, by the heat in which copper melts;—about 90 by the welding heat of iron; about 160, by the greateſt heat that can be produced with coaked pit-coal in a well conſtructed common air-furnace, about eight inches ſquare, ſtill continuing bibulous, ſo as to ſtick to the tongue: ſuch gages, and pieces of ſuch clay, ſo adjuſted, will always compoſe correſpondent thermometers.

Having mentioned occaſionally ſeveral alternate periods of dilatation and contraction in clay, it may be proper to ſtate, and bring into one view, the whole ſucceſſion of changes which I have obſerved in this curious material; as otherwiſe they might create ſome confuſion in the minds of thoſe who have not had occaſion to think attentively on this ſubject, and lead them to aſk how a body ſo variable, and liable to ſuch oppoſite changes from different degrees of heat, can yet be a juſt meaſure of thoſe degrees.

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* Or the diviſions on the ſide may be continued to 300; and in that caſe, inſtead of the widths of the two ends being in proportion of the odd numbers 5 and 3, the one will be juſt double to the other.

The changes which take place in all the natural clays that have come under my examination are ſix.

1. The firſt is, the *ſhrinking* of the moiſt clay in drying, from the mere loſs of its water. The purer the clay is, the more water it requires to ſoften it, and the more it diminiſhes in bulk by the loſs of that water.

2. The dry clay, gradually heated, preſerves its bulk unvaried up to the approach of ignition. At this period it is *enlarged* a little; probably, as already obſerved, from its combined air endeavouring to eſcape.

3. When this air has made its eſcape, the clay begins to diminiſh, or to *loſe the bulk it had before acquired*; and returns back, ſooner or later, to the ſame dimenſions which it was of when dry. It is at this point that the thermometric diminution commences.

4. From this point the clay continues to *diminiſh* more and more in proportion as the heat is increaſed. This I call the *thermometric ſtage* of diminution: it is of greater or leſs extent, terminating at different periods of heat, according to the nature of the clay: in the ſtandard thermometer clay, it commences with viſible ignition, and continues to (doubtleſs far beyond) the extreme heats of our furnaces, an interval conſiſting of 160 degrees of the ſcale: in others, it begins 4, 6, and in ſome even 15 of thoſe degrees later, and terminates alſo much ſooner: and in ſome its whole extent is not above 20 of the ſame degrees. Throughout the greateſt part of this ſtage, the clays are found to retain their property of ſticking to the tongue and imbibing water: between this *bibulous* ſtate and the *vitreſcent* there is an intermediate one, diſtinguiſhed by the name of *porcelain*; and to the higher term of this porcelain ſtate the ſtage of thermometric diminution ſeems to continue.

5. When

5. When the clay has paſſed the porcelain ſtate, it begins to be *enlarged* again, a ſymptom of the vitreſcent ſtage being commenced; and in this period it ſwells more or leſs, according to the nature of its compoſition.

6. By further heat the ſwelled maſs, becoming fluid, ſubſides, is converted into glaſs or ſlag, and *contracted into leſs volume* than the clay occupied in any of its preceding ſtates.

It is plain, therefore, that clay can be a meaſure of heat no further than from ignition, or that point beyond ignition where the third ſtage terminates, to the beginning of the vitreſcent ſtage; and that, as the three firſt changes are completely paſſed before the clay is applied to thermometric purpoſes, being ſtrictly no other than preparatory proceſſes, the thermometer-pieces, whatever *clay* they may be made of (provided it is ſufficiently unvitreſcible), are to be conſidered as poſſeſſing only the fourth ſtage. But a ſingular property of the *compoſition* of clay and alum earth remains to be mentioned, *viz.* that it has really no other than this one ſtage: it ſuffers no enlargement of its bulk at ignition, or in any other period; but proceeds in one uninterrupted courſe of diminution, from the ſoft ſtate in which the pieces are formed, up to the extreme fires of our furnaces. Though the diminution, however, is uninterrupted, it is at the ſame time ſo inconſiderable at the beginning, from the heat of boiling water (at which the pieces are adjuſted) up to ignition, that the ſame point of viſible redneſs is taken for the commencement of the ſcale, in this as in the original clay, without any ſenſible error or variation in their progreſs.

I am inclined to believe, though experiments have not yet enabled me to ſpeak with certainty on this point, that the ſame cauſe which enlarges the *natural clays* on their firſt expoſure to the fire, operates alſo in this *compoſition*, but in a much lower degree; that while the natural *clays* have their whole maſs diſtended by the efforts of the air in forcing its paſſage, the *compoſition* is only reſtrained in its diminution, or prevented from diminiſhing ſo faſt as it otherwiſe would do, and as it is found to do in the ſubſequent part of its courſe, after the air has eſcaped from it.

As the compoſition of clay and alum earth is far more tenacious of water than the clay itſelf, and was found, after being dried by the heat of boiling water, to yield, by diſtillation in a retort, above three times as much aqueous fluid as the original thermometric clay did; it ſeems probable, that a part of this water, retained to the approach of ignition, and in a ſtate of chemical combination, may facilitate the paſſage of the air, ſerving as a vehicle to convey it off through interſtices not permeable to air alone, and conſequently enabling it to eſcape without doing that violence to the maſs, which the natural clays ſuſtain from the expulſion of their air after the water has been detached from it; for the experiments of Dr. PRIESTLEY have ſhewn, that veſſels even of burnt clay are permeable to air when they have imbibed water into their ſubſtance, though not at all ſo in a dry ſtate.

I have now communicated the reſult of a ſeries of experiments which have taken conſiderable time, attention, and labour to complete. Whether the importance of the object will juſtify me in troubling this illuſtrious Society with ſo minute a detail of the moſt material operations, and their reſults, is not for me to determine. If the thermometer ſhould not yet be brought to the perfection that may be wiſhed, I flatter myſelf that ſome abler hand may now take up the ſubject to more advantage; and that philoſophers and artiſts will not be leſs ſucceſsful in ſupplying what may ſtill be deficient, and in aſcertaining, by the *contraction of argillaceous matter*, the meaſurements and effects of the various degrees through the immenſe extent of luminous fire, than they have been with reſpect to the limited and narrow compaſs of low heat, which is meaſurable by the *expanſion of fluids*.

Fig. 4.

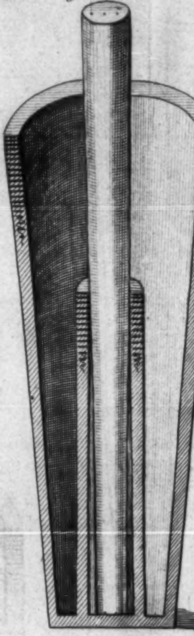


Fig. 1.

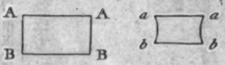
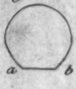


Fig. 2.



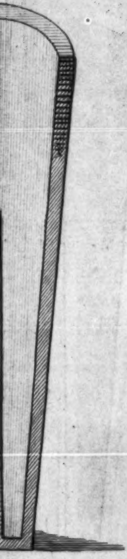
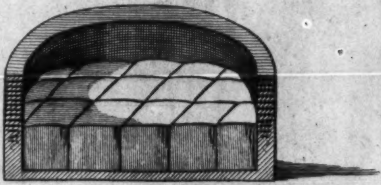


Fig. 3.



Basire Sc.

XXIII. *The Latitude and Longitude of* York *determined from a Variety of Aſtronomical Obſervations; together with a Recommendation of the Method of determining the Longitude of Places by Obſervations of the Moon's Tranſit over the Meridian. Contained in a Letter from* Edward Pigott, *Eſq. to* Nevil Maſkelyne, *D. D. F. R. S. and Aſtronomer Royal.*

Read June 29, 1786.

SIR, Bootham, York, March 16, 1786.

THE great number and variety of obſervations I have made for determining the longitude and latitude of York will, I believe, ſettle thoſe points very accurately: I therefore wiſh to have them preſented to the Royal Society, and beg the favour of you to be at that trouble. The inſtruments I uſed were a good gridiron pendulum clock, a 2½ feet reflector, an eighteen-inch quadrant by BIRD, and a tranſit inſtrument made by SISSON.

The difference of meridians between Greenwich and York was found by the following methods.

Occultations

Occultations of ſtars by the moon.

1783	App. time. h.	′	″	
Sept. 10	11	34	44½	York, immerſion of a ſtar of the ninth magnitude during the eclipſe of the moon; good
	11	49	39½	Paris, at L'Obſervatoire de la Marine; ditto. Ditto, by M. Messier, who determined its R.A. 349° 22′ 17″ and ſouth declination 5° 27′ 54″.
Oct. 7	14	26	28½	York, immerſion of φ Aquarii, inſtantaneous.
	14	37	15½	Greenwich, ditto.
Dec. 30	8	1	24	York, immerſion of δ Piſcium, inſtantaneous: I find I wrote down the minute wrong, it is here corrected.
	8	2	56½	Greenwich, immerſion of ditto.

Mr. Goodricke was ſo obliging as to be at the trouble of computing theſe occultations, and ſent me the reſults as follows:

		′ ″	
By the ſtar of the ninth magnitude		4 29	Difference of meridians between Greenwich and York.
By φ Aquarii		4 23	
By δ Piſcium compared to the Greenwich obſervation	4′ 30″	4 29	
Ditto, to Mr. Wollaston's obſervation,	4 28		
On a mean		4 27	

Obſerved meridian R.A.'s of the moon's limb.

In 1783 this method of finding the difference of meridians occurred to me, and I wrote to Mr. Bayley, your late Aſſiſtant, for information, being entirely ignorant it had ever been noticed; but have ſince ſeen, Sir, that you mention it in your valuable Inſtructions for the Obſervations of the Tranſit of Venus, annexed to the Nautical Almanac of 1769. I have alſo juſt peruſed on the ſame ſubject Abbé Toaldo's ingenious pamphlet written in 1784, which you were ſo kind as to ſend me.

me. Still I find that the great exactneſs of this method is not ſuſpected; I therefore ſhall, in the latter part of this Paper, enter into ſome neceſſary detail, being convinced that, in a very ſhort time, it muſt be univerſally adopted, having every advantage over Jupiter's firſt ſatellite, and but little inferior in preciſion to occultations.

Difference of our meridians by each obſervation.

		′	″			′	″
1781,	Dec. 20.	4	36	1784,	July 2.	4	23
	Dec. 29.	4	10		Nov. 20.	4	23
1782,	June 17.	4	25		Dec. 20.	4	27
	Nov. 30.	4	20		Dec. 22.	4	20
	Dec. 18.	4	25	1785,	Mar. 19.	4	25
1783,	Nov. 3.	4	32		Aug. 16.	4	22
	Dec. 6.	4	39:		Aug. 18.	4	36
	Dec. 30.	4	16		Sept. 12.	4	35
1784,	May 1.	4	8:		Sept. 17.	4	25
	May 25.	4	11		Nov. 12.	4	34
					Nov. 14.	4	18

4′ 24″¼ on a mean.

Obſervations of Jupiter's firſt Satellite.

Dates, &c.	App. time. h.	′	″	
1782, June 3.	12	36	48	York, it immerged near Jupiter.
	12	51	9	Paris, M. Mechain.
Immerſions.	12	51	7	Paris, M. Cassini.
	13	57	40	Buda, Father Weiss.

Obſervations of Jupiter's firſt Satellite continued.

Dates, &c.	App. time			
	h.	′	″	
1782, July 21	9	35	10	York.
	9	39	21	Greenwich, Dr. MASKELYNE.
Emerſions.	9	48	54	Paris, M. MECHAIN; high wind.
	9	48	46	Paris, M. CASSINI.
	10	55	15	Buda, Father WEISS; moon very near Jupiter.
1783, July 3	12	9	50	York; it immerged near Jupiter.
Immerſions.	12	14	20	Greenwich.
	12	24	8	Paris, M. MECHAIN.
Sept. 17	9	48	15	York.
	9	47	44	York, Mr. GOODRICKE; very good.
Emerſions.	9	46	39	Oxford, Mr. HORNSBY.
	10	1	0	Paris, M. MECHAIN; very good.
1784, Aug. 4	10	10	55	York; tolerably good.
Immerſions.	10	10	57	York, Mr. GOODRICKE; middling.
	10	24	57	Paris, M. MECHAIN; air a little hazy.
Sept. 3	14	39	52	York; emerged near Jupiter.
Emerſions.	14	53	51	Paris; thinks rather too late.
Sept. 5	9	8	54	York; good.
	9	13	15	Greenwich, Dr. MASKELYNE.
Emerſions.	9	22	18	Paris, M. MECHAIN; 6 feet reflector, magnifying 450 times.
	9	22	45	Paris; with a 3½ tripl. object glaſs achromatic.
Sept. 12	11	6	9	York; good.
	11	6	24	York, Mr. GOODRICKE; very good.
Emerſions.	11	10	42	Greenwich, Dr. MASKELYNE.
	11	19	47	Paris, M. MECHAIN; as on the 5th.
	11	19	50	Paris, M. MECHAIN; as on the 5th.
1785, July 15	13	37	32	York; good.
Immerſions.	13	42	1	By tables corrected by the obſervations of Greenwich and Marſeilles of July 31, 1785.
July 31	11	53	18	York; good.
Immerſions.	11	57	32	Greenwich; air very clear.
	12	18	53	Marſeilles, M. BERNARD.

Obſer-

Obſervations of Jupiter's firſt Satellite continued.

Dates, &c.	App. time.			
	h.	′	″	
1785, Aug. 30	14	2	59	York; excellent; air remarkably clear.
Immerſions.	14	7	3	Greenwich; ditto.
	14	28	33	Marſeilles, M. BERNARD.
Sept. 15	12	25	2	York; good.
	12	25	4	York, Mr. GOODRICKE; good; moon-light.
Immerſions.	12	29	23	Greenwich; air clear.
	12	50	46	Marſeilles, M. BERNARD.
Nov. 18	7	58	6	York.
Emerſions.	8	2	39	Greenwich; air very clear.
	8	12	2	Paris, M. MECHAIN; a thin cloud.
Dec. 2	11	44	24	York; Jupiter rather low.
Emerſions.	11	49	13	Greenwich; ditto; air clear.

By letters from M. MECHAIN, Buda is 1 h. 6′ 33″ eaſt of Paris, and Marſeilles alſo eaſt 0 h. 12′ 7″.

I obſerved with a 2½ feet reflector, which I believe to be about 10″ of time inferior to the teleſcopes of Greenwich, Oxford, Paris, and Buda. As for Marſeilles no inſtrument is mentioned; therefore, except for that place, 10″ muſt be added to my immerſions, and the ſame ſubtracted from the emerſions; then the difference of meridians between Greenwich and York will be as follows, when each of the obſervations is compared to mine, and a mean thereof taken.

Immerſions.		′	″	
1782,	June 3.	4	54	
1783,	July 3.	4	36	
1784,	Aug. 4.	4	36	
1785,	July 15.	4	19	
	July 31.	4	8	
	Aug. 30.	4	3	
	Sept. 15.	4	16	
		4	24½	on a mean

Emerſions.		′	″
1782,	July 21.	4	29
1782,	Sept. 17.	4	8
1784,	Sept. 3.	4	53
	Sept. 5.	4	33
	Sept. 12.	4	37
1785,	Nov. 18.	4	46
	Dec. 12.	4	59
		4	38

Therefore, by a mean of the immerſions and emerſions, York is 4′ 31″ weſt of Greenwich. Mr. GOODRICKE's emerſion of Sept. 17, 1783, is uſed inſtead of mine, it being undoubtedly more exact.

To enter into any detail concerning the eclipſes of Jupiter's ſatellites would be uſeleſs, as it is a matter ſo amply conſidered by every aſtronomer. I ſhall only ſay that the exactneſs expected even from thoſe of the firſt ſatellite is, in my opinion, too highly rated. Among the various objections, there is one I have often experienced, and which proceeds ſolely from the diſpoſition of the eye, that of ſeeing more diſtinctly at one time than at another. It may not be improper alſo to mention, that the obſervation I ſhould have relied on as the beſt, that of Auguſt 30, 1785, marked excellent, and air remarkably clear both at Greenwich and York, is one of thoſe which differ the moſt from the truth. This I remark without having the moſt diſtant inclination of drawing any concluſion; a ſingle inſtance can be of no weight.

Part of the eclipſe of the Moon, Sept. 10, 1783.

The two laſt columns ſhew the difference of meridians between Greenwich and York. The obſervations marked with an aſteriſk were made by Mr. Goodricke.

Spots obſerved.	York, by Mr. Goodricke and me. App. time	Paris, by M. Mechain. App. time.	Paris, by M. Meſsier. App. time.	Diff. of meridians by M. Mechain.	Diff. of merid. by M. Meſsier.
	h. ′ ″	h. ′ ″	h. ′ ″	′ ″	′ ″
Galileus biſected –	9 45 32	9 58 33	– –	3 38	– –
Ariſtarchus covered –	9 49 13*	10 2 38	– –	4 2	– –
Copernicus touches	9 57 3*	10 11 18	10 11 9	4 52	4 48
	9 57 20	10 11 18	10 11 9	4 35	4 31
Copernicus biſected	9 58 33*	10 12 5	– –	4 9	– –
	9 58 55	10 12 5	– –	3 47	– –
Copernicus covered –	9 59 9*	10 12 57	10 12 41	4 25	4 14
Plato touches –	10 5 7	10 18 37	10 18 40	4 7	4 15
Plato covered –	10 6 18	10 19 52	10 19 28	4 11	3 52
Manilius touches –	10 11 26*	10 25 34	10 25 24	4 45	4 40
	10 11 33	10 25 34	10 25 24	4 38	4 33
Tycho touches –	10 11 57*	10 25 34	10 25 24	4 14	4 9
	10 11 57	10 25 34	10 25 24	4 14	4 9
Manilius covered –	10 12 47*	10 26 29	10 26 53	4 19	4 48
Tycho covered –	10 13 8*	10 27 19	10 27 8	4 48	4 42
	10 13 32	10 27 19	10 27 8	4 24	4 18
Menelaus biſected –	10 15 41	10 29 19	– –	4 15	– –
Prom. Acut. Cen. covered	10 25 26*	10 39 5	– –	4 16	– –
Proclus biſected –	10 29 00	10 42 28	– –	4 5	– –
Mare Criſium touches	10 30 18*	10 43 56	10 44 00	4 15	4 24
	10 30 18	10 43 56	10 44 00	4 15	4 24
Mare Criſium biſected	10 32 43*	10 46 34	10 46 10	4 28	4 9
Mare Criſium covered	10 35 38*	10 49 11	– –	4 10	– –
Grimaldus emerges –	12 23 30	– –	12 37 5	– –	4 17
Grimaldus biſected –	12 23 44	12 36 48	– –	3 41	– –
Grimaldus emerged	12 23 55*	12 37 25	– –	4 7	– –
	12 23 59:	12 37 25	– –	4 3	– –
Galileus emerges –	12 25 50*	– –	12 39 1	– –	3 53
Galileus biſected –	12 25 56*	12 39 16	– –	3 57	– –
Ariſtarchus biſected.	12 28 59	12 43 8	– –	4 46	– –

Difference of meridians on a mean 4′ 16″

M. Mechain's Obſervatory was 9′ 23″, and M. Messier's 9′ 18″ eaſt of Greenwich.

Thus I have given a comparative view of the different methods I employed in ſettling the longitude of our Obſervatory, which is in Bootham, about 400 or 500 yards N. W. of the Minſter. The occultations and meridian tranſits of the moon's limb, which make it 4′ 25″½, or 1° 6′ 23″, would have been quite ſufficient; but ſtill it is intereſting and uſeful to know how far the others err. With reſpect to the eclipſes of the moon's ſpots, I think that method is in general too much neglected; and that it might be relied on infinitely more, if certain circumſtances were mutually attended to.

1ſt, To be particular in ſpecifying the clearneſs of the ſky; for in hazy weather the reſults are very erroneous.

2dly, To chuſe ſuch ſpots that are well defined, and leave no heſitation as to the part eclipſed.

3dly, That every obſerver ſhould, as much as poſſible, uſe teleſcopes equally powerful; at leaſt let the magnifying powers be the ſame.

A principal objection may ſtill be urged, *viz.* the difficulty of diſtinguiſhing the true ſhadow from the penumbra. Was this obviated, I believe, the reſults would be more exact than from Jupiter's firſt ſatellite: undoubtedly the ſhadow appears better defined if magnified little; but I am much inclined to think, that with high magnifying powers there is greater certainty of chuſing the ſame part of the ſhadow, which perhaps is more than a ſufficient compenſation for the loſs of diſtinctneſs.

Concerning the meridian obſervations of the moon's limb.

The advantages and preciſion of this method for determining the difference of meridians is, as I have already ſaid, ſo little ſuſpected, that I flatter myſelf, the particulars I am going to mention will not be thought ſuperfluous.

The rule I adopted is this:

The increaſe of the moon's R.A. in 12 hours (or any given time) found by computation, is to 12 hours as the increaſe of the moon's R.A. between two places, found by obſervation, is to the difference of meridians.

EXAMPLE.

November 30, 1782.

h.	′	″		
13	12	57,62	meridian tranſit of the moon's ſecond limb	at Greenwich by clock.
13	13	29,08	ditto of α ♍	
		31,46	Difference of R.A.	
13	14	8,05	meridian tranſit of the moon's ſecond limb	at York by clock.
13	14	30,13	ditto of α ♍	
		22,08	difference at York,	the clocks going nearly ſidereal time no correction is required.
		31,46	difference at Greenwich,	
		9,38	increaſe of the moon's apparent R.A. between Greenwich and York, by obſervation.	

141″ in ſeconds of a degree, ditto, ditto, ditto.

The increaſe of the moon's R.A. for 12 hours by computation is 23340 ſeconds,
and 12 hours reduced into ſeconds is . 43200;

therefore, according to the rule ſtated above,

23340″ : 43200″ :: 141″ : difference of meridians = 261″ —

Theſe eaſy obſervations and ſhort reduction are the whole of the buſineſs. Inſtead of computing the moon's R.A. for 12 hours, I have conſtantly taken it from the Nautical Almanacs, which give it ſufficiently exact, provided ſome attention be paid to the increaſe or decreaſe of the moon's motion.

Were the following circumſtances attended to, the reſults would undoubtedly be much more exact.

1ſt, Compare the obſervations to the ſame made in ſeveral other places.

2dly, Let ſeveral and the ſame ſtars be obſerved at theſe places.

3dly, Such ſtars as are neareſt in R.A. and declination to the moon are infinitely preferable.

4thly, Your advice to get as near as poſſible an equal number of obſervations of each limb, to take a mean of each ſet, and then a mean of both means, cannot be too ſtrongly urged. I am perfectly of your opinion, that it will conſiderably correct the error of teleſcopes and ſight.

5thly, The adjuſtment of the teleſcopes to the eye of the obſerver before the obſervation, which you alſo recommend, will appear very judicious to every aſtronomer, who muſt have frequently perceived what you mention, that the ſight is ſubject to vary.

6thly, As a principal error proceeds from the obſervation of the moon's limb, I think it may be conſiderably leſſened, if certain little round ſpots near each limb were alſo obſerved in ſettled Obſervatories; in which caſe the libration of the moon will perhaps be a conſideration.

7thly, When the difference of meridians, or of the latitudes of the places, is very conſiderable, the change of the moon's diameter becomes an equation.

Though ſuch are the requiſites to uſe this method with advantage, only one or two of them have been employed in the obſervations that I have reduced. Two thirds of theſe obſervations had not even the ſame ſtars obſerved at Greenwich and York; and yet none of the reſults, except a doubtful one, differ 15″ from the mean; therefore, I think, we may expect a ſtill greater exactneſs, perhaps within 10″, if the above particulars be attended to.

When the ſame ſtars are not obſerved, it is neceſſary for the obſervers at both places to compute their R.A. from tables, in order to get the apparent R.A. of the moon's limb; though this is not ſo ſatisfactory as by actual obſervation, ſtill the difference will be trifling, provided the ſtars R.A.'s are accurately ſettled. Your catalogue undoubtedly may be depended on the moſt, and thoſe ſtars preferred which have their proper motions aſcertained. A few years ago, I had the pleaſure of communicating to you the proper motion of β Virginis, which I found to be 1″,02 *per* year, increaſing in R.A.*: was this unknown, and that ſtar obſerved alone with the moon, it would occaſion, at this time, a very conſiderable error.

I am alſo of opinion, that the ſame method can be put in practice by travellers with little trouble, and a tranſit inſtrument conſtructed ſo as to fix up with facility in any place. Though I have not conſidered this ſufficiently, I ſhall, nevertheleſs, ſubjoin a few remarks that may engage others to turn their thoughts more fully to the ſubject.

It is not neceſſary, perhaps, that the inſtrument ſhould be perfectly in the meridian to a few ſeconds of time, provided ſtars, nearly in the *ſame parallel of declination* with the moon, are obſerved: nay, I am inclined to think, that if the inſtrument deviates even a quarter or half of a degree, or more, ſufficient exactneſs can be obtained, as a table might be computed,

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* Some time previous to this communication, I had found, by the compariſon of my tranſit obſervations of α Aquilæ and β Virginis, that the latter had moved forward with a proper motion of 0″,91 of time, or of 13″,65 of R.A. from 1767 to 1783, in 16 years, or at the rate of 0″,853 a year, on ſuppoſitiou that the proper motion of α Aquilæ is 0″,57 a year forward.

puted, ſhewing the moon's parallax and motion for ſuch deviation, which deviation may eaſily be found by the well known method of obſerving ſtars whoſe difference of declination is conſiderable.

As travellers very ſeldom meet with ſituations to obſerve ſtars near the pole, or find a proper object for determining the error of the line of collimation, I ſhall recommend the following idea, which, I believe, has never yet been noticed, and hope it will anſwer the purpoſe. Having computed the apparent R.A. of four, ſix, or more ſtars, which have nearly the ſame parallel of declination, obſerve half of them with the inſtrument inverted, and the other half when in its right poſition; if the difference of R.A.'s between each ſet by obſervation agree with the computation, there is no error; but if they diſagree, half that diſagreement is the error of the line of collimation. The ſame obſervations may alſo ſerve to determine whether the diſtance of the correſponding wires are equal. In caſe of neceſſity, each limb of the ſun might be obſerved in the ſame manner, though probably with leſs preciſion. By a ſingle trial I made above two years ago, the reſult was much more exact than I expected. MAYER's Catalogue of Stars will prove of great uſe to thoſe that adopt the above method.

In ſuch a number of obſervations, it is not ſurpriſing that a few ſhould be erroneous; I have rejected only three.

A meridian tranſit of the moon's limb, Auguſt 18, 1782; δ Sagitarii was the only ſtar obſerved at York; it gives for difference of meridians, . . . ′ ″ 3 55

Perhaps the ſtar has a proper motion, or a miſtake of one ſecond might have been made in marking the clock.

An immerſion of Jupiter's firſt ſatellite, June 22, 1783, which make the difference of meridians, . 3 42

The air was hazy both at Greenwich and at York.

Laſtly, an occultation of a ſtar of the ninth magnitude, immerged behind the dark limb of the moon, during the eclipſe of Sept 10, 1783, at 11 h. 29′ 6″ apparent time. M. Messier alſo obſerved it at 11 h. 50′ 49″¼ apparent time at Paris: he determined its R.A. 349° 22′ 17″, and declination 5° 38′ 23″ ſouth. M. Goodricke, who computes very accurately, finds it gives for difference of meridians, . . . 4 44¾

I am rather ſurpriſed, that the immerſions of known ſtars of the ſixth and ſeventh magnitude behind the *dark limb* of the moon are not conſtantly obſerved in fixed Obſervatories, as they would frequently be of great uſe.

Latitude of York.

The following determinations for the latitude of York were made with a Bird's 18-inch quadrant, the teleſcope of two feet focus, with which inſtrument obſervations of the ſame ſtar ſeldom differ 10″.

Latitude of the Obſervatory.

°	′	″			
53	57	37	by 7	obſervations	of Arcturus.
53	57	41	by 2	ditto	of α Lyræ.
53	57	52	by 1	ditto	of β Arietis.
53	57	37	by 1	ditto	of β Cygni.
53	57	33	by 2	ditto	of Algol.
53	57	57	by 4	ditto	of γ Lyræ.
53	57	49	by 8	ditto	of β Draconis.
53	57	46	by 6	ditto	of μ Draconis.
53	57	56	by 2	ditto	of γ Draconis,
53	57	45+	latitude on a mean.		

The line of collimation was deduced from β, γ, and μ Draconis; half of each ſet obſerved with the face of the quadrant to the eaſt, and half with its face to the weſt. This, as well as the other methods, is very tedious, particularly when required to be often repeated, as is the caſe in travelling; I ſhall therefore propoſe the following invention, the idea of which was improved on by Mr. SMEATON, and flatter myſelf it will prove of the greateſt facility.

The error of the line of collimation includes the fixed errors of the inſtrument, and thoſe that are ſubject to change, occaſioned by the wires and glaſſes, &c. of the teleſcope moving. The error of theſe laſt may be found by making the teleſcope turn on its center, ſo that the ſun, ſtars, or terreſtrial objects may be obſerved on the horizontal wire in two manners; firſt, when the wire is in its natural poſition, and then inverted, which is performed by turning the teleſcope 180 degrees, or half round: thus, this part of the error can always be known with the greateſt eaſe; and in order to find the fixed errors, it is requiſite for *a ſingle time* to get the *whole error* of the line of collimation by one of the common methods, from which the error of the teleſcope being deducted, the fixed errors become known; and as they are unchangeable, if any alteration ſhould take place, it proceeds from the teleſcope, and may eaſily be detected as ſhewn above. Perhaps, inſtead of the whole teleſcope, it would be ſufficient only to make that part turn containing the eye-glaſs and wires.

As the following obſervations made alſo at York may be of uſe, I beg, Sir, you will annex them to my paper on the longitude and latitude of that city, which lately I had the pleaſure of ſending you.

Dates.	App. time.			
	h.	′	″	
1781, July 19	9	41	59	Emerſion of Jupiter's ſecond ſatellite; night fine.
1782, May 24	12	23	12	Immerſion of Jupiter's ſecond ſatellite; good.
July 20	11	27	40:	Emerſion of Jupiter's 2d ſat.; doubtful; air very hazy.
Nov. 30	20	57	16	Immerſion of α ♍ behind the moon; inſtantaneous.
	20	57	21½*	Ditto ditto; in another part of the town.
				Eclipſe of the moon.
1783, Mar. 18	8	27	50	Total immerſion of the moon; air very clear.
	8	27	33*	Ditto; good.
	10	9	36	Moon begins to emerge; } air hazy.
	10	10	18	Certainly emerged;
June 26	13	35	21	Immerſion of Jupiter's ſecond ſatellite; good.
	13	34	52*	Ditto; middling.
				Eclipſe of the moon; air clear.
Sept. 10	9	30	45	Appearance of penumbra.
	12	17	30	Moon not emerged, but light ſtrong.
	12	19	35	Ditto; very ſtrong.
	12	21	14	Moon begins to emerge, but uncertain.
	12	21	44	Ditto; more certain.
	12	21	56*	Ditto; ditto.
	12	22	24	Moon certainly emerged.
	12	22	24*	Ditto.
	13	21	00	End of the eclipſe, doubtful; air hazy.
	13	21	23*	Ditto.
	13	22	18*	Certainly ended, but not clear of penumbra.
	13	22	45	Ditto, ditto; air clearer.
				Several ſpots were obſerved, but are here omitted, for fear of being too voluminous.
Sept. 16	10	22	41	Emerſion of Jupiter's ſecond ſatellite; air clear; but Jupiter low.
23	9	27	18	Emerſion of Jupiter's 3d ſat.; Jupiter low; undulation.
Oct. 11	7	34	9*	Emerſion of Jupiter's ſecond ſatellite.
	7	34	21	Ditto; tolerably good.
29	5	42	53	Emerſion of Jupiter's third ſatellite.
	5	46	16	Equal in brightneſs to the ſecond ſatellite; air clear.
1784, July 27	10	7	46	Immerſion of Jupiter's third ſatellite; tolerably good, though undulation.

Dates.	App. time. h.	′	″		
1784, Aug. 26	8	54	12	Immerſion of τ ♐ behind the moon; inſtantaneous.	
Oct. 11	9	49	30	Emerſion of Jupiter's ſecond ſatellite; good, though ſlight haze.	
	9	49	26*	Ditto.	
Nov. 12	9	33	59	Emerſion of Jupiter's ſecond ſatellite.	
	9	34	1*	Ditto.	
1785, July 15	12	26	50	Immerſion of Jupiter's ſecond ſatellite; air clear.	
Aug. 18	11	44	37	Immerſion of Jupiter's third ſatellite; good;	the air a little vapouriſh.
Sept. 17	12	16	55	Immerſion of Jupiter's ſecond ſatellite; good;	
Oct. 29	6	33	26	Emerſion of Jupiter's third ſatellite;	
Nov. 15	9	24	±	I examined Jupiter's fourth ſatellite during 20′, without being certain whether it had diminiſhed in light.	
Dec. 15	5	50	48	Immerſion of 125 ♉ by the moon, exact within 3″.	

I have again marked with an aſteriſk the obſervations made by Mr. GOODRICKE, who deſired me to communicate them. This worthy young man exiſts no more; he is not only regretted by many friends, but will prove a loſs to aſtronomy, as the diſcoveries he ſo rapidly made ſufficiently evince: alſo his quickneſs in the ſtudy of mathematics was well known to ſeveral perſons eminent in that line.

Declination of the needle.

	h		°	′	
1780, Sept. 13.	at 2½,	by a mean of 22 trials,	23	40½	Declination weſt.
1782, Dec. 26.	at 0¾,	by a mean of 16 trials,	23	5+	
1783, Nov. 14.	at 0¾,	by a mean of 19 trials,	23	59−	
1784, Jan. 17.	at 0⅔,	by a mean of 13 trials,	23	54+	

Theſe obſervations were taken with all poſſible exactneſs; the needle was four inches long, and made by DOLLOND.

Sir H. ENGLEFIELD, when at Scarborough, in Auguſt and September, 1781, was ſo kind as to obſerve, at noon, the height of his barometer and thermometer. I alſo made ſimilar obſervations in the Obſervatory at York; from which, by eight compariſons, none diſagreeing above 0,018 of an inch from the mean, I find, that the quickſilver at the ſea ſtood 0,063 of an inch higher than at York. The barometers were made by RAMSDEN, and they agreed together to 0,005 part of an inch. We may later alſo expect to get the mean height of the barometer and thermometer, as there are ſeveral gentlemen that obſerve them every day, particularly Mr. WYVIL and Dr. WHITE at York, and Mr. CHOMONLEY at Branſby.

I remain, Sir, with great regard, &c.

EDW. PIGOTT.

May 26, 1786.

XXIV. *Advertiſement of the expected Return of the Comet of 1532 and 1661 in the Year* 1788. *By the Rev.* Nevil Maſkelyne, *D. D. F. R. S. and Aſtronomer Royal.*

Read June 29, 1786.

THE comet of 1531, 1607, and 1682, having returned in the year 1759, according to Dr. HALLEY's prediction in his *Synopſis Aſtronomiæ Cometicæ*, firſt publiſhed in the Philoſophical Tranſactions in 1705, and re-publiſhed with his Aſtronomical Tables in 1749, there is no reaſon to doubt that all the other comets will return after their proper periods, according to the remark of the ſame author.

In the firſt edition of the *Synopſis* he ſuppoſed the comets of 1532 and 1661, from the ſimilarity of the elements of their orbits, to be one and the ſame; but in the ſecond edition he has ſeemed to leſſen the weight of his firſt conjecture by not repeating it. Probably he thought it beſt to eſtabliſh this new point in aſtronomy, the doctrine of the revolution of comets in elliptic orbits, as all philoſophical matters in the beginning ſhould be, on the moſt certain grounds; and feared that the vague obſervations of the comet, made by APIAN in 1532, might rather detract from, than add to, the evidence ariſing from more certain *data*. Aſtronomers, however, have generally acquieſced in his firſt conjecture of the comets of 1532 and 1661 being one and the ſame, and to expect its return to its perihelium accordingly in 1789.

The interval between the paſſages of the comet by the perihelium in 1532 and 1661 is 128 years, 89 days, 1 hour, 29 minutes (32 of the years being biſſextile), which added to the time of the perihelium in 1661, together with 11 days to reduce it from the Julian to the Gregorian ſtile, which we now uſe, brings out the expected time of the next perihelium to be April 27th, 1 h. 10′ in the year 1789.

The periodic times of the comet, which appeared in 1531, 1607, and 1682, having been of 76 and 75 years alternately, Dr. Halley ſuppoſed, that the ſubſequent period would be of 76 years, and that it would return in the year 1758; but, upon conſidering its near approach to Jupiter, in its deſcent towards the ſun in the ſummer of 1681, he found, that the action of Jupiter upon the comet was, for ſeveral months together, equal to one-fiftieth part of the ſun upon it, tending to increaſe the inclination of the orbit to the plane of the ecliptic, and lengthen the periodic time. Accordingly, the inclination of the orbit was found by the obſervations made in the following year 1682 to be 22′ greater than in the year 1607. The effect of the augmentation of the periodic time could not be ſeen till the next return, which he ſuppoſed would be protracted by Jupiter's action to the latter end of the year 1758, or the beginning of 1759. M. Clairaut, previous to its return, took the pains to calculate the actions both of Jupiter and Saturn on it during the whole periods from 1607 to 1682, and from 1682 to 1759, and thence predicted its return to its perihelium by the middle of April; it came about the middle of March, only a month ſooner, which was a ſufficient approximation to the truth in ſo delicate a matter, and did honour to this great mathematician, and his laborious calculations.

The comet in queſtion is alſo, from the poſition of its orbit, liable to be much diſturbed both by Jupiter and Saturn, particularly in its aſcent from the ſun after paſſing its perihelium, if they ſhould happen to be near it, when it appoaches to or croſſes their orbits; becauſe it is very near the plane of them at that time. When it paſſed the orbit of Jupiter in the beginning of February 1682, O. S. it was 50° *in conſequentia* of that planet; and when it paſſed the orbit of Saturn in the beginning of October 1663, it was 17° *in conſequentia* of it. Hence its motion would be accelerated while it was approaching towards the orbit of either planet by its ſeparate action, and retarded when it had paſſed its orbit; but, as it would be ſubjected to the effect of retardation through a greater part of its orbit than to that of acceleration, the former would exceed the latter, and conſequently the periodic time would be ſhortened; but probably not much, on account of the conſiderable diſtance of the comet from the planets when it paſſed by them; and therefore we may ſtill expect it to return to its perihelium in the beginning of the year 1789, or the latter end of the year 1788, and certainly ſome time before the 27th of April 1789. But of this we ſhall be better informed after the end of this year, from the anſwers to the prize queſtion propoſed by the Royal Academy of Sciences at Paris, to compute the diſturbances of the comet of 1532 and 1661, and thence to predict its return *.

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* Since this was written, I received the unwelcome news, in a letter from M. MECHAIN, of the Royal Academy of Sciences at Paris, that the Academy has not received ſatisfactory anſwers concerning the diſturbances of the comet between 1532 and 1661, and 1661 and the approaching return, and that the prize is referred to be adjudged of at Eaſter 1788, and that it will be 6000 livres. N. M.

If it ſhould come to its perihelium on the 1ſt of January 1789, it might probably be viſible, with a good achromatic teleſcope, in its deſcent to the ſun, the middle of September 1788, and ſooner or later, according as its perihelium ſhould be ſooner or later. It will approach us from the ſouthern parts of its orbit, and therefore will firſt appear with conſiderable ſouth latitude and ſouth declination; ſo that perſons reſiding nearer the equator than we do, or in ſouth latitude, will have an opportunity of diſcovering it before us. It is to be wiſhed that it may be firſt ſeen by ſome aſtronomer in ſuch a ſituation, and furniſhed with proper inſtruments for ſettling its place in the heavens, the earlieſt good obſervations being moſt valuable for determining its elliptic orbit, and proving its identity with the comets of 1532 and 1661. The Cape of Good Hope would be an excellent ſituation for this purpoſe.

In order to aſſiſt aſtronomers in looking out for this comet, I have here given its heliocentric and geocentric longitudes and latitudes and correſpondent diſtances from the ſun and earth, on ſuppoſition that it ſhall come to its perihelium on January 1, 1789. But if that ſhould happen ſooner or later, the heliocentric longitudes and latitudes and diſtances from the ſun will ſtand good if applied to days as much earlier or later, as the time of the perihelium may happen ſooner or later; and the geocentric longitudes and latitudes and diſtances from the earth muſt be re-computed accordingly. The calculations are made for a parabolic orbit from the elements determined by Dr. HALLEY from HEVELIUS's obſervations in 1661, only allowing for the preceſſion of the equinoxes. The elements made uſe of were as follows:

Time of perihelium January 1, 1789, at noon.

Perihelium diſtance 0,44851.

Place

Place of aſcending node $2^{s}\ 24^{\circ}\ 18'$.

Inclination of orbit to the ecliptic $32^{\circ}\ 36'$.

Perihelium forwarder in orbit than the aſcending node $33^{\circ}\ 28'$.

It motion is direct.

Computed places of the comet, on ſuppoſition that it ſhall return to its perihelium January 1, 1789, at noon.

Times.	Dist. from ⊙.	Dist. from the earth.	Heliocentric longitude.			Heliocentric latitude.			Geocentric longitude.			Geocentric latitude.			Product of diſtances from ⊙ and earth.
1788			S.	D.	M	D.	M.		S.	D.	M.	D.	M.		
Apr. 23, 7	4, 0	4,52	11	3	54	30	56	S	11	16	30	27	5	S	18,07
June 4, 1	3, 5	3,54	11	7	6	31	25		11	26	31	31	4		12,38
July 14, 5	3,	2,57	11	11	16	31	55		0	3	21	38	11		7,70
Aug. 2,46	2,75	2,15	11	13	47	32	10		0	4	8	42	59		5,90
— 20,43	2, 5	1,79	11	16	39	32	22		0	2	0	48	16		4,48
Sept. 7, 3	2,25	1,51	11	20	9	32	32		11	25	6	53	28		3,39
— 24, 0	2,	1,29	11	24	16	32	36		11	13	12	56	45		2,58
Oct. 10,26	1,75	1,13	11	29	24	32	30		10	28	22	56	36		1,75
— 26,64	1,50	1,01	0	5	51	32	4		10	15	50	52	6		1,51
Nov. 9,34	1,25	0,88	0	14	19	31	0		10	8	36	46	47		1,10
— 23,30	1, 0	0,76	0	26	4	28	32		10	4	10	39	0		0,76
Dec. 7,21	0,75	0,62	1	13	58	22	29		9	29	18	27	45		0,46
— 23,32	0,50	0,50	2	20	58	2	8		9	14	31	2	7	S	0,25
— 24,35	0,49	0,51	2	24	18	0	0		9	12	58	0	0		0,25
1789															
Jan. 1, 0	0,45	0,59	3	23	25	17	17	N	9	2	50	13	8	N	0,26

The laſt obſervation made by **HEVELIUS** on the comet in 1661 was when its diſtance from the earth was 0,986, and from the ſun 1,37, with what he calls a very long and good teleſcope; at which time it appeared faint and ſmall with it, though ſtill ſufficiently viſible. Let us ſuppoſe this to have been a teleſcope of 9-feet focal length, with an aperture of 1,65 inch; then, becauſe the diameter of the aperture of a teleſcope ſufficient to render the comet equally viſible ſhould be as the product of its diſtances from the ſun and earth, and the product of the numbers above-mentioned 0,986 and 1,37 is 1,35, we ſhall have the following analogy to find the aperture of a refracting teleſcope ſufficient to ſhew the comet as it appeared to HEVELIUS. As 1,35 : 1,65 inch :: 9 : 11 inches, ſo is the product of diſtances from the ſun and earth to the diameter of the aperture required in inches.

XXV. *A new Method of finding Fluents by Continuation. By the Rev.* Samuel Vince, *A. M. F. R. S.*

Read July 6, 1786.

ART. I. Put $\dot{F} = \frac{x^m\dot{x}}{a^n+x^n} = x^{m-n}\dot{x} - a^n x^{m-2n}\dot{x} + a^{2n}x^{m-3n}\dot{x} -$ &c. $\pm \frac{a^{vn}x^{m-vn}\dot{x}}{a^n+x^n}$, then $F = \frac{x^{m-n+1}}{m-n+1} - \frac{a^n x^{m-2n+1}}{m-2n+1} + \frac{a^{2n}x^{m-3n+1}}{m-3n+1} -$ &c. $\pm W$, where W repreſents the fluent of the laſt term. Now $\frac{x^m\dot{x}}{\overline{a^n+x^n}|^r} = \frac{x^m\dot{x} \times \overline{a^n+x^n}|^{1-r}}{a^n+x^n}$; hence $\int \frac{x^m\dot{x}}{\overline{a^n+x^n}|^r} = \int \frac{x^m\dot{x} \times \overline{a^n+x^n}|^{1-r}}{a^n+x^n} = F \times \overline{a^n+x^n}|^{1-r} - \int F \times \frac{\overline{1-r}\,.\,nx^{n-1}\dot{x}}{\overline{a^n+x^n}|^r} =$ (by ſubſtituting for F its value in the latter quantity) $F \times \overline{a^n+x^n}|^{1-r} - \frac{\overline{1-r}\,.\,n}{m-n+1} \times \int \frac{x^m\dot{x}}{\overline{a^n+x^n}|^r} + \frac{\overline{1-r}\,.\,na^n}{m-2n+1} \times \int \frac{x^{m-n}\dot{x}}{\overline{a^n+x^n}|^r} - \frac{\overline{1-r}\,.\,na^{2n}}{m-3n+1} \times \int \frac{x^{m-2n}\dot{x}}{\overline{a^n+x^n}|^r} +$ &c. $\pm \int W \times \frac{\overline{1-r}\,.\,nx^{n-1}\dot{x}}{\overline{a^n+x^n}|^r}$; tranſpoſe $-\frac{\overline{1-r}\,.\,n}{m-n+1} \times \int \frac{x^m\dot{x}}{\overline{a^n+x^n}|^r}$ and divide both ſides of the equation by $\frac{m-rn+1}{m-n+1}$ and we have $\int \frac{x^m\dot{x}}{\overline{a^n+x^n}|^r} = \frac{m-n+1}{m-rn+1} \times F \times \overline{a^n+x^n}|^{1-r} + \frac{m-n+1}{m-rn+1} \times \frac{\overline{1-r}\,.\,na^n}{m-2n+1} \times \int \frac{x^{m-n}\dot{x}}{\overline{a^n+x^n}|^r} - \frac{m-n+1}{m-rn+1} \times \frac{\overline{1-r}\,.\,na^{2n}}{m-3n+1} \times \int \frac{x^{m-2n}\dot{x}}{\overline{a^n+x^n}|^r} +$ &c. $\pm \frac{m-n+1}{m-rn+1} \times$

$\int W \times \frac{\overline{1-r}\,.\,nx^{n-1}\dot{x}}{\overline{a^n+x^n}^{\,r}}$; now the fluent of the laſt term is $\mp\frac{m-n+1}{m-rn+1} \times W \times \overline{a^n+x^n}^{\,1-r} \pm \frac{m-n+1}{m-rn+1} \times \int \frac{a^{vn}x^{m-vn}\dot{x}}{\overline{a^n+x^n}^{\,r}}$; hence by ſubſtituting this quantity for the laſt term, it is manifeſt, that the firſt part $\mp \frac{m-n+1}{m-rn+1} \times W \times \overline{a^n+x^n}^{\,1-r}$ will be deſtroyed by the laſt term of $\frac{m-n+1}{m-rn+1} \times F \times \overline{a^n+x^n}^{\,1-r}$, when we ſubſtitute for F its value; hence if we put $M = \frac{x^{m-n+1}}{m-n+1} - \frac{a^n x^{m-2n-1}}{m-2n+1} + \frac{a^{2n}x^{m-3n+1}}{m-3n+1}$ – &c. omitting the laſt term ± W, we have $\int \frac{x^m\dot{x}}{\overline{x^n+x^n}^{\,r}} = \frac{m-n+1}{m-rn+1} \times M \times \overline{a^n+x^n}^{\,1-r} + \frac{m-n+1}{m-rn+1} \times \frac{\overline{1-r}\,.\,na^n}{m-2n+1} \times \int \frac{x^{m-n}\dot{x}}{\overline{a^n+x^n}^{\,r}} - \frac{m-n+1}{m-rn+1} \times \frac{\overline{1-r}\,.\,na^{2n}}{m-3n+1} \times \int \frac{x^{m-2n}\dot{x}}{\overline{a^n+x^n}^{\,r}} + \&c. \pm \frac{m-n+1}{m-rn+1} \times \times a^{vn} \times \int \frac{x^{m-vn}\dot{x}}{\overline{a^n+x^n}^{\,r}}$; hence, if the fluent of the laſt term be given, we have the general law of continuation by which we may find the fluent of $\frac{x^m\dot{x}}{\overline{a^n+x^n}^{\,r}}$. If the fluxion be $\frac{x^m\dot{x}}{\overline{a^n+x^n}^{\,r}}$ all the terms after the firſt will be negative, and the laſt always poſitive.

Ex. 1. Given the fluent of $\frac{\dot{x}}{\sqrt{1+x^2}}$ to find the fluent of $\frac{x^{2s}\dot{x}}{\sqrt{1+x^2}}$.

Here $n=2$, $a=1$, $m=2s$, $r=\frac{1}{2}$, $M = \frac{x^{2n-1}}{2n-1} - \frac{x^{2n-3}}{2n-3}$ + &c. to $\pm x$,

$\pm x$, and the fluent of $\frac{\dot{x}}{\sqrt{1+x^2}}$ is the hyp. log. $x+\sqrt{1+x^2}$ which call Q; hence $\int\frac{x^{2s}\dot{x}}{\sqrt{1+x^2}}=\frac{2s-1}{2s}\times M\times\sqrt{1+x^2}+\frac{2s-1}{2s\,.\,\overline{2s-3}}\times\int\frac{x^{2s-2}\dot{x}}{\sqrt{1+x^2}}-\frac{2s-1}{2s\,.\,\overline{2s-5}}\times\int\frac{x^{2s-4}\dot{x}}{\sqrt{1+x^2}}+\text{\&c.}\pm\frac{2s-1}{2s}\times Q.$

If $s=1, \int\frac{x^2\dot{x}}{\sqrt{1+x^2}}=\frac{1}{2}\sqrt{1+x^2}\times x-\frac{1}{2}Q=\alpha.$

$s=2, \int\frac{x^4\dot{x}}{\sqrt{1+x^2}}=\frac{3}{4}\sqrt{1+x^2}\times\overline{\frac{x^3}{3}-x}+\frac{3}{4}\alpha+\frac{3}{4}Q=\beta.$

$s=3, \int\frac{x^6\dot{x}}{\sqrt{1+x^2}}=\frac{5}{6}\sqrt{1+x^2}\times\overline{\frac{x^5}{5}-\frac{x^3}{3}+x}+\frac{5}{6\,.\,3}\beta-\frac{5}{6}\alpha-\frac{5}{6}Q=\gamma.$

$s=4, \int\frac{x^8\dot{x}}{\sqrt{1+x^2}}=\frac{7}{8}\sqrt{1+x^2}\times\overline{\frac{x^7}{7}-\frac{x^5}{5}+\frac{x^3}{3}-x}+\frac{7}{8\,.\,5}\gamma-\frac{7}{8\,.\,3}\beta+\frac{7}{8}\alpha+\frac{7}{8}Q.$

&c. &c. &c.

Ex. 2. To find the fluent of $x^{\frac{s}{2}}\dot{x}\sqrt{2+x}$, given the fluent of $x^{-\frac{1}{2}}\dot{x}\sqrt{2+x}$, and s an odd number.

Here $a=2$, $n=1$, $r=-\frac{1}{2}$, $\frac{s}{2}=m$, $m-vn=-\frac{1}{2}$, or $\frac{s}{2}-v=-\frac{1}{2}$; $\therefore v=\frac{s+1}{2}$, $M=\frac{2x^{\frac{s}{2}}}{s}-\frac{4x^{\frac{s}{2}-1}}{s-2}+\frac{8x^{\frac{s}{2}-2}}{s-4}-$ &c. and the fluent (Q) of $x^{-\frac{1}{2}}\dot{x}\sqrt{2+x}$ is $\pi+\sqrt{2x+x^2}$, where $\pi=$ hyp. log. $1+x+\sqrt{2x+x^2}$; hence $\int x^{\frac{s}{2}}\dot{x}\sqrt{2+x}=\frac{s}{s+3}\times\overline{2+x}|^{\frac{3}{2}}\times M+\frac{s}{s+3}\times\frac{b}{s-2}\times\int x^{\frac{s}{2}-1}\dot{x}\sqrt{2+x}-\frac{s}{s+3}\times\frac{12}{s-4}\times\int x^{\frac{s}{2}-2}\dot{x}\sqrt{2+x}+\frac{s}{s+3}\times\frac{24}{s-6}\times\int x^{\frac{s}{2}-3}\dot{x}\sqrt{2+x}$ &c. $\pm\frac{s}{s+3}\times 2^{\frac{s+1}{2}}\times\int x^{-\frac{1}{2}}\dot{x}\sqrt{2+x}.$

If

$$\text{If } s=1,\ \int x^{\frac{1}{2}}\dot{x}\sqrt{2+x}=\tfrac{1}{4}\times\overline{2+x}|^{\frac{3}{2}}\times 2x^{\frac{1}{2}}-\tfrac{1}{2}Q=\alpha.$$

$$s=3,\ \int x^{\frac{3}{2}}\dot{x}\sqrt{2+x}=\tfrac{1}{2}\times\overline{2+x}|^{\frac{3}{2}}\times\overline{\frac{2x^{\frac{3}{2}}}{3}-4x^{\frac{1}{2}}}+3\alpha+2Q=\beta.$$

$$s=5,\ \int x^{\frac{5}{2}}\dot{x}\sqrt{2+x}=\tfrac{1}{5}\times\overline{2+x}|^{\frac{3}{2}}\times\overline{\frac{2x^{\frac{5}{2}}}{5}-\frac{4x^{\frac{3}{2}}}{3}+8x^{\frac{1}{2}}}+\frac{5}{4}\beta-\frac{15}{2}\alpha-5Q=\gamma.$$

$$s=7,\ \int x^{\frac{7}{2}}\dot{x}\sqrt{2+x}=\tfrac{7}{10}\times\overline{2+x}|^{\frac{3}{2}}\times\overline{\frac{2x^{\frac{7}{2}}}{7}-\frac{4x^{\frac{5}{2}}}{5}+\frac{8x^{\frac{3}{2}}}{3}-16x^{\frac{1}{2}}}+\frac{21}{25}\gamma-\frac{14}{5}\beta+\frac{84}{5}\alpha+\frac{56}{5}Q.$$

&c. &c. &c.

II. Let $\dot{F}\ \frac{x^n\dot{x}}{a+bx^m+x^{2m}}=x^{n-2m}\dot{x}-Px^{n-3m}\dot{x}+Qx^{n-4m}\dot{x}-\&c.\pm\frac{Vx^{n-sm}\dot{x}}{a+bx^m+x^{2m}}\pm\frac{Wx^{n-\overline{s+1}\cdot m}\dot{x}}{a+bx^m+x^{2m}}$, then $F=\frac{x^{n-2m+1}}{n-2m+1}-\frac{Px^{n-3m-1}}{n-3m+1}+\frac{Qx^{n-4m+1}}{n-4m+1}-\&c.\pm T\pm U$, where T and U are put for the fluents of the two laſt terms, and P, Q, &c. for the co-efficients ariſing from the diviſion. Now, $\int\frac{x^n\dot{x}}{\sqrt{a+bx^m+x^{2m}}}=\int\frac{x^n\dot{x}\times\overline{a+bx^m+x^{2m}}|^{1-r}}{a+bx^m+x^{2m}}=F\times\overline{a+bx^m+x^{2m}}|^{1-r}-\int F+\frac{\overline{1-r}\cdot mbx^{m-1}\dot{x}+\overline{1-r}\cdot 2mx^{2m-1}\dot{x}}{\overline{a+bx^m+x^{2m}}|^{r}}=$ (by ſubſtituting for F its value in the latter quantity, and putting A, B, C, &c for the co-efficients which ariſe in conſequence thereof) $F\times\overline{a+bx^m+x^{2m}}|^{1-r}-A\times\int\frac{x^n\dot{x}}{\overline{a+bx^m+x^{2m}}|^{r}}+B\times\int\frac{x^{n-m}\dot{x}}{\overline{a+bx^m+x^{2m}}|^{r}}-C\times\int\frac{x^{n-2m}\dot{x}}{\overline{a+bx^m+x^{2m}}|^{r}}+\&c.$

$$\mp\int \mathrm{T}\times\frac{\overline{1-r}\,.\,mbx^{m-1}\dot{x}+\overline{1-r}\,.\,2mx^{2m-1}\dot{x}}{\overline{a+bx^{m}+x^{2m}}|^{r}}$$

$\mp\int \mathrm{U}\times\frac{\overline{1-r}\,.\,mbx^{m-1}\dot{x}+\overline{1-r}\,.\,2mx^{2m-1}\dot{x}}{\overline{a+bx^{m}+x^{2m}}|^{r}}$; hence by tranſpoſition and diviſion we have $\int\frac{x^{n}\dot{x}}{\overline{a+bx^{m}+x^{2m}}|^{r}}=\frac{1}{1+\mathrm{A}}\times \mathrm{F}\times\overline{a+bx^{m}+x^{2m}}|^{1-r}$ $+\;\frac{\mathrm{B}}{1+\mathrm{A}}\times\int\frac{x^{n-m}\dot{x}}{\overline{a+bx^{m}+x^{2m}}|^{r}}-\frac{\mathrm{C}}{1+\mathrm{A}}\times\int\frac{x^{n-2m}\dot{x}}{\overline{a+bx^{m}+x^{2m}}|^{r}}+$ &c.

$$\mp\int\frac{\mathrm{T}}{1+\mathrm{A}}\times\frac{\overline{1-r}\,.\,mbx^{m-1}\dot{x}+\overline{1-r}\,.\,2mx^{2m-1}\dot{x}}{\overline{a+bx^{m}+x^{2m}}|^{r}}$$

$\mp\int\frac{\mathrm{U}}{1+\mathrm{A}}\times\frac{\overline{1-r}\,.\,mbx^{m-1}\dot{x}+\overline{1-r}\,.\,2mx^{2m-1}\dot{x}}{\overline{a+bx^{m}+x^{2m}}|^{r}}$. Now the fluents of theſe two laſt terms are $\mp\frac{\mathrm{T}}{1+\mathrm{A}}\times\overline{a+bx^{m}+x^{2m}}|^{1-r}\pm\frac{\mathrm{V}}{1+\mathrm{A}}$ $\int\frac{x^{n-3m}\dot{x}}{\overline{a+bx^{m}+x^{2m}}|^{r}}$ and $\mp\frac{\mathrm{U}}{1+\mathrm{A}}\times\overline{a+bx^{m}+x^{2m}}|^{1-r}\pm\frac{\mathrm{W}}{1+\mathrm{A}}\int\frac{x^{n-\overline{3+1}\,.\,m}\dot{x}}{\overline{a+bx^{m}+x^{2m}}|^{r}}$ reſpectively; hence, by ſubſtituting theſe for the laſt term, it is manifeſt that $\mp\frac{\mathrm{T}}{1+\mathrm{A}}\times\overline{a+bx^{m}+x^{2m}}|^{1-r}$ and $\mp\frac{\mathrm{U}}{1+\mathrm{A}}$ $\times\overline{a+bx^{m}+x^{2m}}|^{1-r}$ will be deſtroyed by the two laſt terms of $\frac{1}{1+\mathrm{A}}\times \mathrm{F}\times\overline{a+bx^{m}+x^{2m}}|^{1-r}$ when we ſubſtitute for F its value; hence, if we put $\mathrm{M}=\frac{x^{n-2m-1}}{n-2m+1}-\frac{\mathrm{P}x^{n-3m+1}}{n-3m+1}+\frac{\mathrm{Q}x^{n-4m+1}}{n-4m+1}-$ &c. omitting the two laſt terms $\pm$ T and $\pm$ U, we ſhall have $\int\frac{x^{n}\dot{x}}{\overline{a+bx^{m}+x^{2m}}|^{r}}=\frac{1}{1+\mathrm{A}}\times \mathrm{M}\times\overline{a+bx^{m}+x^{2m}}|^{1-r}+\frac{\mathrm{B}}{1+\mathrm{A}}\times$ $\int\frac{x^{n-m}\dot{x}}{\overline{a+bx^{m}+x^{2m}}|^{r}}-\frac{\mathrm{C}}{1+\mathrm{A}}\times\int\frac{x^{n-2m}\dot{x}}{\overline{a+bx^{m}+x^{2m}}|^{r}}+$ &c. $\pm\frac{\mathrm{V}}{1+\mathrm{A}}\times$

$\int\frac{x^{n-sm}\dot{x}}{\overline{a+bx^m+x^{2m}}^{\,r}}\pm\frac{W}{1+A}\times\frac{x^{n-\overline{s+1}.m}\dot{x}}{\overline{a+bx^m+x^{2m}}^{\,r}}$; hence if the two laſt fluents be given, we have the general law of continuation up to $\frac{x^n\dot{x}}{\overline{a+bx^m+x^{2m}}^{\,r}}$ in the ſame manner as before.

III. In general, if we proceed as in the two laſt articles, we ſhall find $\int\frac{x^n\dot{x}}{\overline{a+bx^m+\&c.\ x^{tm}}^{\,r}}=\frac{M}{P}\times\overline{a+bx^m+\&c.\ x^{tm}}^{\,1-r}+\frac{A}{P}\times\int\frac{x^{n-m}\dot{x}}{\overline{a+bx^m+\&c.\ x^{tm}}^{\,r}}+\frac{B}{P}\times\int\frac{x^{n-2m}\dot{x}}{\overline{a+bx^m+\&c.\ x^{tm}}^{\,r}}+\&c.\pm\frac{T}{P}\times\int\frac{x^{n-sm}\dot{x}}{\overline{a+bx^m+\&c.\ x^{tm}}^{\,r}}\pm\frac{V}{P}\times\int\frac{x^{n-\overline{s+1}.m}\dot{x}}{\overline{a+bx^m+\&c.\ x^{tm}}^{\,r}}\pm\&c.$ where the number of theſe laſt terms is t, and $M=\frac{x^{n-tm+1}}{n-tm+1}-\frac{Qx^{n-\overline{t+1}.m-1}}{n-\overline{t+1}.m+1}$ + &c. omitting, as before, the terms at the end ariſing from the remainders. Hence if the laſt t fluents be given, we can by continuation find the required fluent.

Becauſe the diviſion of $\frac{x^n\dot{x}}{a+bx^m+\&c.\ x^{tm}}$ may be expreſſed by an *aſcending* ſeries $x^n\dot{x}-Qx^{n+m}\dot{x}+Rx^{n+2m}\dot{x}-\&c.$ it is manifeſt, that by the ſame method we may continue the fluents downwards as well as upwards.

IV. Let $\dot{F}=\frac{x^n\dot{x}}{1-x}=-x^{n-1}\dot{x}-x^{n-2}\dot{x}-x^{n-3}\dot{x}-\&c.-x^{n-r}\dot{x}+\frac{x^{n-r}\dot{x}}{1-x}$, then $F=-\frac{x^n}{n}-\frac{x^{n-1}}{n-1}-\frac{x^{n-2}}{n-2}-\&c.-\frac{x^{n-r+1}}{n-r+1}+W$, where W is the fluent of the laſt term. Now $\frac{x^n\dot{x}}{\sqrt{1-x^2}}=\frac{x^n\dot{x}}{1-x}\times\sqrt{\frac{1-x}{1+x}}$, hence $\int\frac{x^n\dot{x}}{\sqrt{1-x^2}}=\int\frac{x^n\dot{x}}{1-x}\times\sqrt{\frac{1-x}{1+x}}=\mathrm{F}\times\sqrt{\frac{1-x}{1+x}}+\int\mathrm{F}\times$ $\frac{\dot{x}}{\sqrt{\overline{1+x^2}}\times\overline{1+x}}=\mathrm{F}\times\sqrt{\frac{1-x}{1+x}}-\int\frac{x^n\dot{x}}{n\sqrt{\overline{1-x^2}}\times\overline{1+x}}-\int\frac{x^{n-1}\dot{x}}{\overline{n-1}\,.\,\sqrt{\overline{1-x^2}}\times\overline{1+x}}$ $-\int\frac{x^{n-2}\dot{x}}{\overline{n-2}\,.\,\sqrt{\overline{1-x}}\times\overline{1+x}}-\text{\&c.}-\int\frac{x^{n-r+1}\dot{x}}{\overline{n-r+1}\,.\,\sqrt{\overline{1-x^2}}\times\overline{1+x}}+$ $\int\mathrm{W}\times\frac{\dot{x}}{\sqrt{\overline{1-x^2}}\times\overline{1+x}}$. But

$$\frac{x^n\dot{x}}{n\sqrt{\overline{1-x^2}}\times\overline{1+x}}=\frac{x^{n-1}\dot{x}}{n\sqrt{1-x^2}}-\frac{x^{n-2}\dot{x}}{n\sqrt{1-x^2}}+\frac{x^{n-3}\dot{x}}{n\sqrt{1-x^2}}-\text{\&c.}\mp\frac{x^{n-r}\dot{x}}{n\sqrt{1-x^2}}\pm\frac{x^{n-r}\dot{x}}{n\sqrt{\overline{1-x^2}}\times\overline{1+x}}$$

$$\frac{x^{n-1}\dot{x}}{\overline{n-1}\,.\,\sqrt{\overline{1-x^2}}\times\overline{1+x}}=\frac{x^{n-2}\dot{x}}{\overline{n-1}\,.\,\sqrt{1-x^2}}-\frac{x^{n-3}\dot{x}}{\overline{n-1}\,.\,\sqrt{1-x^2}}+\text{\&c.}\pm\frac{x^{n-r}\dot{x}}{\overline{n-1}\,.\,\sqrt{1-x^2}}\mp\frac{x^{n-r}\dot{x}}{\overline{n-1}\,.\,\sqrt{\overline{1-x^2}}\times\overline{1+x}}$$

$$\frac{x^{n-2}\dot{x}}{\overline{n-2}\,.\,\sqrt{\overline{1-x^2}}\times\overline{1+x}}=\frac{x^{n-3}\dot{x}}{\overline{n-2}\,.\,\sqrt{1-x^2}}-\text{\&c.}\mp\frac{x^{n-r}\dot{x}}{\overline{n-2}\,.\,\sqrt{1-x^2}}\pm\frac{x^{n-r}\dot{x}}{\overline{n-2}\,.\,\sqrt{\overline{1-x^2}}\times\overline{1+x}}$$

&c. &c. &c.

$$\frac{x^{n-r+1}\dot{x}}{\overline{n-\ +1}\,.\,\sqrt{\overline{1-x^2}}\times\overline{1+x}}=\frac{x^{n-r}\dot{x}}{\overline{n-r+1}\,.\,\sqrt{1+x^2}}-\frac{x^{n-r}\dot{x}}{\overline{n-r+1}\,.\,\sqrt{\overline{1-x^2}}\times\overline{1+x}}.$$

Hence $\int\frac{x^n\dot{x}}{\sqrt{1-x^2}}=\mathrm{F}\times\sqrt{\frac{1-x}{1+x}}-\frac{1}{n}\times\int\frac{x^{n-1}\dot{x}}{\sqrt{1-x^2}}+\overline{\frac{1}{n}-\frac{1}{n-1}}\times$ $\int\frac{x^{n-2}\dot{x}}{\sqrt{1-x^2}}-\overline{\frac{1}{n}-\frac{1}{n-1}+\frac{1}{n-2}}\times\int\frac{x^{n-3}\dot{x}}{\sqrt{1-x^2}}+\text{\&c.}\,\overline{\pm\frac{1}{n}\mp\frac{1}{n-1}\pm\text{\&c.}}$ $\overline{-\frac{1}{n-r+1}}\times\int\frac{x^{n-r}\dot{x}}{\sqrt{1-x^2}}\overline{\mp\frac{1}{n}\pm\frac{1}{n-1}\mp\text{\&c.}+\frac{1}{n-r+1}}\times\int\frac{x^{n-r}\dot{x}}{\sqrt{\overline{1-x^2}}\times\overline{1+x}}$ $+\int\mathrm{W}\times\frac{\dot{x}}{\sqrt{\overline{1-x^2}}\times\overline{1+x}}$. Now $\int\mathrm{W}\times\frac{\dot{x}}{\sqrt{\overline{1-x^2}}\times\overline{1+x}}=-\mathrm{W}\times$ $\overline{\frac{1-x}{1+x}}\Big|^{\frac{1}{2}}+\int\frac{x^{n-r}\dot{x}}{\sqrt{1-x^2}}$; alſo $\int\frac{x^{n-r}\dot{x}}{\sqrt{\overline{1-x^2}}\times\overline{1+x}}=-x^{n-r}\times\sqrt{\frac{1-x}{1+x}}+$ $\overline{n-r}\times\int\frac{x^{n-r-1}\dot{x}}{\sqrt{1-x^2}}-\overline{n-r}\times\int\frac{x^{n-r}\dot{x}}{\sqrt{1-x^2}}$; hence, by ſubſtituting theſe quantities in the two laſt terms, it is manifeſt that $-\mathrm{W}\times\overline{\frac{1-x}{1-x}}\Big|^{\frac{1}{2}}$ will be deſtroyed by the laſt term of

F ×

$F \times \left.\overline{\frac{1-x}{1+x}}\right|^{\frac{1}{2}}$ when we ſubſtitute for F its value; therefore, if we put $M = -\frac{x^n}{n} - \frac{x^{n-1}}{n-1} - \frac{x^{n-2}}{n-2} - \&c. - \frac{x^{n-r+1}}{n-r+1}$, we ſhall have

$$\int \frac{x^n \dot{x}}{\sqrt{1+2}} = \overline{M \pm \frac{1}{n} \mp \frac{1}{n-1} \pm \&c. - \frac{1}{n-r+1}} \times \left.\overline{\frac{1-x}{1+x}}\right|^{\frac{1}{2}} - \frac{1}{n} \times \int \frac{x^{n-1}\dot{x}}{\sqrt{1-x^2}}$$

$$+ \overline{\frac{1}{n} - \frac{1}{n-1}} \times \int \frac{x^{n-2}\dot{x}}{\sqrt{1-x^2}} - \overline{\frac{1}{n} - \frac{1}{n-1} + \frac{1}{n-2}} \times \int \frac{x^{n-3}\dot{x}}{\sqrt{1-x^2}} + \&c.$$

$$\overline{\pm \frac{1}{n} \mp \frac{1}{n-1} \pm \&c. - \frac{1}{n-r+1}} \times \overline{n-r+1} + 1 \times \int \frac{x^{n-r}\dot{x}}{\sqrt{1-x^2}} \mp$$

$$\overline{\frac{1}{n} \pm \frac{1}{n-1} \mp \&c + \frac{1}{n-r+1}} \times \overline{n-r} \times \int \frac{x^{n-r-1}\dot{x}}{\sqrt{1-x^2}}.$$

Hence, if the two laſt fluents be given, we have the general law of continuation up to the fluent of $\frac{x^n \dot{x}}{\sqrt{1-x^2}}$, where the index of x without the vinculum increaſes by unity each time. And in the ſame manner we may (by increaſing the index of x without by m) find the fluent of $\frac{x^n \dot{x}}{\sqrt{a^{2m} - x^{2m}}}$ if we have given the fluents of $\frac{x^{n-rm}\dot{x}}{\sqrt{a^{2m} - x^{2m}}}$ and $\frac{x^{\overline{n-r+1}\,.\,m}\dot{x}}{\sqrt{a^{2m} - x^{2m}}}$. Thus we have a general law of continuation, where the index of x without is increaſed by half the index under the vinculum.

V. Aſſume $\dot{F} = \frac{x^n \dot{x}}{x^m - b} = x^{n-m}\dot{x} + bx^{n-2m}\dot{x} + b^2 x^{n-3m}\dot{x} + \&c. + \frac{b^r x^{n-rm}\dot{x}}{x^m - b}$, then $F = \frac{x^{n-m+1}}{n-m+1} + \frac{bx^{n-2m+1}}{n-2m+1} + \frac{b^2 x^{n-3m+1}}{n+3m+1} + \&c. + W$, where W is put for the fluent of the laſt term. Now $\int x^n \dot{x} \sqrt{\frac{x^m - a}{x^m - b}} = \int \frac{x^n \dot{x}}{x^m - b} \times \sqrt{\overline{x^m - a} \times \overline{x^m - b}} = F \times \sqrt{\overline{x^m - a} \times \overline{x^m + b}}$

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$-\int F\times\frac{2mx^{2m-1}\dot{x}-\overline{a+b}\,.\,mx^{m-1}\dot{x}}{2\sqrt{\overline{x^m-a}\times\overline{x^m-b}}}$ = (by ſubſtituting for F its value in the latter quantity, and putting A, B, C, &c. for the co-efficients which ariſe therefrom) $F\times\sqrt{\overline{x^m-a}\times\overline{x^m-b}}-A\int\frac{x^n\dot{x}}{\sqrt{\overline{x^m-a}\times\overline{x^m-b}}}-B\int\frac{x^{n-m}\dot{x}}{\sqrt{\overline{x^m-a}\times\overline{x^m-b}}}-C\int\frac{x^{n-2m}\dot{x}}{\sqrt{\overline{x^m-a}\times\overline{x^m-b}}}-$ &c. $-\int W\times\frac{2mx^{2m-1}\dot{x}-\overline{a+b}\,.\,mx^{m-1}\dot{x}}{2\sqrt{\overline{x^m-a}\times\overline{x^m-b}}}$. Now $\frac{x^n\dot{x}}{\sqrt{\overline{x^m-a}\times\overline{x^m-b}}}=\frac{x^n\dot{x}}{x^m-a}\sqrt{\frac{x^m-a}{x^m-b}}$, $\frac{x^{n-m}\dot{x}}{\sqrt{\overline{x^m-a}\times\overline{x^m-b}}}=\frac{x^{n-m}\dot{x}}{x^m-a}\sqrt{\frac{x^m-a}{x^m-b}}$, $\frac{x^{n-2m}\dot{x}}{\sqrt{\overline{x^m-a}\times\overline{x^m-b}}}=\frac{x^{n-2m}\dot{x}}{x^m-a}\sqrt{\frac{x^m-a}{x^m-b}}$, &c. But

$$\frac{x^n\dot{x}}{x^m-a}=x^{n-m}\dot{x}+ax^{n-2m}\dot{x}+a^2x^{n-3m}\dot{x}+\&c.+a^{r-1}x^{n-rm}\dot{x}+\frac{a^rx^{n-rm}\dot{x}}{x^m-a}$$

$$\frac{x^{n-m}\dot{x}}{x^m-a}=\qquad x^{n-2m}\dot{x}+ax^{n-3m}\dot{x}+\&c.+a^{r-2}x^{n-rm}\dot{x}+\frac{a^{r-1}x^{n-rm}\dot{x}}{x^m-a}$$

$$\frac{x^{n-2m}\dot{x}}{x^m-a}=\qquad x^{n-3m}\dot{x}+\&c.+a^{r-3}x^{n-rm}\dot{x}+\frac{a^{r-2}x^{n-rm}\dot{x}}{x^m-a}$$

&c. &c. &c.

Hence, by ſubſtitution, $\int x^n\dot{x}\sqrt{\frac{x^m-a}{x^m-b}}=F\times\sqrt{\overline{x^m-a}\times\overline{x^m-b}}$ $-A\int x^{n-m}\dot{x}\sqrt{\frac{x^m-a}{x^m-b}}-\overline{Aa+B}\times\int x^{n-2m}\dot{x}\sqrt{\frac{x^m-a}{x^m-b}}-\overline{Aa^2+Ba+C}$ $\times\int x^{n-3m}\dot{x}\sqrt{\frac{x^m-a}{x^m-b}}-$ &c. $-\overline{Aa^{r-1}+Ba^{r-2}+Ca^{r-3}+\&c.}\times$ $\times\int x^{n-rm}\dot{x}\sqrt{\frac{x^m-a}{x^m-b}}-\overline{Aa^r+Ba^{r-1}+Ca^{r-2}+\&c.}\times\int\frac{x^{n-rm}\dot{x}}{\sqrt{\overline{x^m-a}+\overline{x^m-b}}}$ $-\int W\times\frac{2mx^{2m-1}\dot{x}-\overline{a+b}\,.\,mx^{m-1}\dot{x}}{2\sqrt{\overline{x^m-a}\times\overline{x^m-b}}}$. But

$-\int \mathrm{W} \times \frac{2mx^{2m-1}\dot{x} - \overline{a+b}.mx^{m-1}\dot{x}}{2\sqrt{\overline{x^m-a}\times\overline{x^m-b}}}$ is $-\mathrm{W}\times\sqrt{\overline{x^m-a}\times\overline{x^m-b}}$ $+b^r\times\int x^{n-rm}\dot{x}\sqrt{\frac{x^m-a}{x^m-b}}$; hence, by ſubſtituting this for the laſt term, it is manifeſt, that $-\mathrm{W}\times\sqrt{\overline{x^m-a}\times\overline{x^m-b}}$ will be deſtroyed by the laſt term of $\mathrm{F}\times\sqrt{\overline{x^m-a}\times\overline{x^m-b}}$ when we ſubſtitute for F its value; therefore, if we put $\mathrm{M}=\frac{x^{n-m+1}}{n-m+1}+\frac{bx^{n-2m+1}}{n-2m+1}+\&c.+\frac{b^{r-1}x^{n-rm+1}}{n-rm+1}$, we have $\int x^n\dot{x}\sqrt{\frac{x^m-a}{x^m-b}}=\mathrm{M}\times\sqrt{\overline{x^m-a}\times\overline{x^m-b}}-\mathrm{A}\int x^{n-m}\dot{x}\sqrt{\frac{x^m-a}{x^m-b}}-\overline{\mathrm{A}a+\mathrm{B}}\times\int x^{n-2m}\dot{x}\sqrt{\frac{x^m-a}{x^m-b}}-\overline{\mathrm{A}a^2+\mathrm{B}a+\mathrm{C}}\times\int x^{n-3m}\dot{x}\sqrt{\frac{x^m-a}{x^m-b}}-\&c.-\overline{\mathrm{A}a^{r-1}+\mathrm{B}a^{r-2}+\&c.-b^r}\times\int x^{n-rm}\dot{x}\sqrt{\frac{x^m-a}{x^m-b}}-\overline{\mathrm{A}a^r+\mathrm{B}a^{r-1}+\mathrm{C}a^{r-2}+\&c.}\times\int\frac{x^{n-rm}\dot{x}}{\sqrt{\overline{x^m-a}\times\overline{x^m-b}}}$. Hence if the laſt two fluents be given, we have the general law of continuation up to the fluent of $x^n\dot{x}\sqrt{\frac{x^m-a}{x^m-b}}$.

The utility of finding fluents by continuation was manifeſt to Sir ISAAC NEWTON, who firſt propoſed it; and ſince his time ſome of the moſt eminent mathematicians have employed much of their attention upon it. The method which I have inveſtigated and exemplified in this Paper I offer as being entirely new; and at the ſame time it not only exhibits, at once, the general law up to the required fluent, but alſo appears, from ſome of the inſtances here given, to be more extenſive and convenient in its application than any method hitherto offered.

The general reſolution of the given fluxion into a ſeries of fluxions of the ſame kind, where the index of the unknown quantity without the vinculum keeps decreaſing or increaſing either by the index under or by half the index, has not, that I know of, before been given; which furniſhes us at once not only with a very eaſy method of *continuing* fluents, but alſo points out a very ſimple method of inveſtigating the fluent of the given fluxion *without* continuation. For if $\int \dot{A} = p + b\int \dot{B} + c\int \dot{C} + d\int \dot{D} + \&c.$ $\int \dot{B} = p' + c'\int \dot{C} + d'\int \dot{D} + \&c.$ $\int \dot{C} = p'' + d''\int \dot{D} + \&c.$ &c. &c. then if for $\int \dot{B}$, $\int \dot{C}$, &c. &c. we ſubſtitute their reſpective values, we ſhall get a general ſeries for $\int \dot{A}$ without continuation. The extent of any new method is, at firſt, ſeldom obvious; and how far that which is here propoſed may be ſucceſsfully employed in other caſes will beſt appear from its application. Different methods will always be found to have their uſes in particular caſes; for where one becomes impracticable another will often be found to ſucceed; and I hope that which is here offered will contribute ſomething towards facilitating the inveſtigation of fluents.

XXVI. *Conjectures relative to the Petrifactions found in* St. Peter's *Mountain, near* Maeſtricht. *By* Petrus Camper, *M. D. F. R. S.*

Read July 6, 1786.

THE diſcovery of a great number of petrified bones about the year 1770, in the mountain of St. Peter at Maeſtricht, and particularly of large jaw-bones with their teeth, ſuggeſted to the late M. HOFFMAN, firſt Surgeon to the Military Hoſpital at Maeſtricht, a worthy member of ſeveral learned Societies, and a great admirer of natural hiſtory, the idea that theſe maxillæ belonged to crocodiles. This notion was ſpread by himſelf and his literary correſpondents through all Europe.

He did me the favour to ſend me, not only the hiſtory of thoſe petrifactions, but alſo ſeveral figures of the jaw-bones in queſtion, and of other bones, which were all intirely new to me, except ſome fragments of the bones of turtles. I diſcovered, however, at the very firſt ſight, the characteriſtical differences which diſtinguiſhed theſe bones from thoſe of crocodiles, of which I had at that time ſeveral in my collection.

His intention was to write upon this ſubject, and to ſend his eſſay, containing his reaſons for ſuppoſing theſe bones to belong to crocodiles, to the Royal Society; but I diſſuaded him, as a friend, from doing this, leſt he ſhould afterwards be under a neceſſity of retracting his opinion: and I ſent him a figure of the lower jaw of a crocodile, accurately done by my own hand, and ſoon after the ſkull and under jaw of a pretty large crocodile; which induced him to defer his deſign of writing about theſe antiquities of the old world, until he ſhould be better informed on the ſubject of cetaceous fiſhes.

Major DROUIN, of Maeſtricht, who made, about the ſame time, a collection of an infinite variety of corals, madrepores, alcyoniums, echinites, belemnites, ſhells, and petrified wood, from the ſame mountain and its environs, likewiſe procured a beautiful ſpecimen of two maxillary bones of the ſame incognitum, but with the inſides turned outwards; and this gentleman alſo ſuppoſed them to belong to the crocodile. A ſketch of this ſpecimen is to be found in M. BUCHOZ's *Dons de la Nature*, tab. 68. But the ſpecimen itſelf is now in TEYLER's Muſeum, at Haerlem, with the whole of Major DROUIN's collection.

Another ſtill more valuable and perfect ſpecimen is to be ſeen at the houſe of the reverend Dean GODDING, of which there is likewiſe a rough ſketch in M. BUCHOZ's *Dons de la Nature*, pl. 66. In this the greater part of both the upper and under maxillary bones is intire, and a bone, with ſmall teeth, belonging to the palate; by which it appears, the animal had not only teeth in the jaw-bones, but alſo in the throat, as ſeveral fiſhes have, but which are never found in the mouth of crocodiles.

Notwithſtanding all my endeavours to convince my friends, and afterwards M. DROUIN, and particularly the Dean, whoſe valuable and truly beautiful ſpecimens I ſaw in the year 1782, I never could prevail upon them to adopt my opinion, that theſe bones belonged to phyſeteres or reſpiring fiſhes. M. HOFFMANN, adhering cloſely to the Linnæan Syſtem, objected,

jected, that the phyſeteres had teeth only in the lower jawbone, whereas this foſſil monſter had them in both upper and lower maxilla. He did not ſeem to recollect, that φυσητὴρ ſignifies ſomething reſpiring, or breathing, and applied to fiſhes, *breathing fiſhes*; nor that the phyſeteres, according to the Linnæan ſyſtem, have ſmall teeth in the upper jaw-bone, though larger ones in the lower jaw, according to the obſervations of Dr. OTHO FABRICIUS, in his Fauna Groenlandica, p. 42. where he mentions the *macrocephalus*, and p. 45. where he ſpeaks of the *microps*.

In Auguſt 1782, I ſent M. GODDING, who had favoured me with a copy of his valuable ſpecimen, a full demonſtration of its being the head of a phyſeter, or breathing fiſh, Delphinus, or Orca, or under whatever genus it may be ranked, as having large teeth of the ſame ſize in both the maxillæ. But in vain; for he continues ſtill to call it a crocodile, as if its value depended upon the ſpecies of the animal.

The analogy of all the other marine bodies ſeems to make it ſtill more probable, that theſe large bones belong to the inhabitants of the ſea, and not of rivers. The large turtles, the numberleſs echinites, madrepores, ſhells, alcyoniums, belemnites, orthoceratites, and ſo on, are all ſea animals; and the crocodile would, in that caſe, be the only inhabitant of the rivers mixed with them.

The pretended crocodile found near Whitby, in Yorkſhire, Phil. Tranſ. vol. L. p. II. 1758, § 92. p. 688. and ibid. § 108. p. 786. is undoubtedly the ſkeleton of a Balæna.

§ 2. After the deceaſe of M. HOFFMAN, his family having offered the whole collection for ſale, I went in Auguſt 1782 to Maeſtricht on purpoſe to examine it; and I could not but greatly admire the richneſs and beauty of the collection, eſpecially that of the foſſil bones from St. Peter's mountain; but as the heirs did not conſider the expences neceſſary to tranſport the collection down the Maeſe, where each ſovereign puts an enormous duty upon every thing that paſſes through his territories, nor the ſmall number of perſons who were likely to purchaſe it, they rated the price ſo high that nobody choſe to bid for it.

The eldeſt daughter having at length become poſſeſſed of the whole, offered me the principal ſpecimens at a price I agreed to. Amongſt them were the duplicates I have already ſent to the Britiſh Muſeum, and with which the honourable Truſtees are perfectly ſatisfied. Theſe ſpecimens may ſerve likewiſe to aſcertain what I have ſaid about them, as being real fragments of phyſeteres, ſome of turtles, and the like, but not a ſingle one of any ſpecies of crocodile.

§ 3. The arguments for their being jaw-bones and vertebræ of fiſhes ſeem to be, firſt, the ſmoothneſs of theſe bones; and, ſecondly, the many holes by which the nerves go out at the ſide, and under each tooth, as is very evident in that beautiful ſpecimen now in the Britiſh Muſeum, on the outſide of which eleven holes are viſible, in the ſame manner as they are in the delphini, and more particularly in the lower jaw-bone of the cete, the Phyſeter macrocephalus, or pot-fiſh, cachalot, &c. Thirdly, the form of the teeth, which have ſolid roots, as in tab. XV. fig. 6. B, C, E, F, and the ſix teeth of tab. XVI. Fourthly, becauſe there are little teeth in the palate, as in Dean GODDING's ſpecimen. Fifthly, becauſe the vertebræ have the appearance of true cetaceous vertebræ, as in fig. 5. tab. XV. and in ſeveral beautiful and large ſpecimens now in the Muſeum. Several of theſe vertebræ were beſides intirely unknown to me, and not at all analogous to the vertebræ of the crocodile, deſcribed and repreſented by Dr. N. Grew.

§ 4. As I intended to viſit London in 1785, I flattered myſelf I ſhould ſtill find the ſkeleton of the great crocodile formerly at Greſham College, and be able to find out ſuch characteriſtic diſtinctions as ſhould be neceſſary to decide the queſtion. Dr. Gray was ſo kind as to go with me to the lower apartments of the Britiſh Muſeum, where we found, though not without difficulty, the ſkeleton much neglected, ſpoiled, and deprived of ſeveral intereſting parts. I admired, nevertheleſs, the remainder of it, being infinitely pleaſed with the tranſverſe ſutures, tab. XV. fig. 1, 2. *a*, *b*. *c*, *f*. δ, ζ. by which not only thoſe of the neck and thorax, but thoſe of the loins alſo, are divided, and which I made a drawing of, as large as the life, the 20th of October, 1785, of which fig. 1. and 2. are very accurate copies.

I confeſs I had not obſerved that particular diviſion or ſuture in the ſkeleton of a ſmall crocodile, of thirteen inches, made by my youngeſt ſon; but after being apprized of it by the large ſkeleton in the Muſeum, of twelve feet four inches, Paris meaſure, on looking at my own when I returned home, I found them both alike, and that thoſe parts were not epiphyſes; of which, however, the tranſverſe proceſſes of the neck. fig. 1. *d*, *e*, *q*, *o*, *n*, *p*, have all the appearance, though there is no other epiphyſis to be obſerved in the reſt of the bones of that large ſkeleton.

When we compare the foſſil vertebra, fig. 5. with thoſe now in the Muſeum, we ſhall find the epiphyſes AB, CD, analogous to *a*, *b*, *c*, *d*, fig. 4. being the real epiphyſes in the vertebra of a young porpoiſe.

I procured, in London, the largeſt vertebræ of the neck of a turtle I could get, and prepared two of them as in fig. 3. in which, as along the back of that ſingular creature, I found the tranſverſe diviſions *a*, *e*, *d*, *f*: of all which I have not ſeen a ſingle inſtance amongſt the dorſal ſpinæ from St. Peter's mountain, one of which conſiſts of ſeven, another of twelve, and a third of fourteen vertebræ. Some of the vertebræ have, I acknowledge, an inferior proceſs, as in the crocodile, *l*, *m*, fig. 1. Of theſe I have ſent likewiſe two to the Muſeum. The oſtrich, and the turtle *Mydas*, have ſuch proceſſes, but no quadruped I know of.

The articulation of the vertebræ with each other, by the ſurfaces of the bodies themſelves, is intirely different, not only from that of the crocodile, but from that of all the cetaceous fiſhes I have ever ſeen: and I dare venture to aſſert, I have ſeen a great many, excluſive of thoſe in my collection. The anterior part of the Maeſtricht vertebræ is more or leſs triangular and hollow, as in fig. 5. C, D, L. The poſterior A B is convex. Both theſe ſurfaces are very ſmooth, as if they had been covered with a very thin cartilage, and moved one upon the other, without being united by an elaſtic lamella, as in all quadrupeds and cetaceous fiſhes; in which the vertebræ have on both the ſurfaces a round brim, or circular edge, *a*, *b*, *i*, *b*, by means of which the ligaments are connected, and a flat hollow ſurface within, as *b*, *i*, fig. 4. for the elaſtic pulp that is between them.

§ 5. The dentition is ſo ſingular in theſe foſſil jaw-bones that it deſerves a particular deſcription. In all quadrupeds, as in man, the teeth which appear firſt are all ſhed at a certain period of life, and in the mean time new ones are formed above, under, or at the ſides of the primordial or temporary teeth, but in different ſockets. The grinders are not all renewed, but in general three when there are ſix, and two when there are five. Nature, however, is not always uniform in this operation. Mr. JOHN HUNTER, a worthy Member of our Society, has given a very intereſting and complete natural hiſtory of the teeth, in which theſe obſervations are ſtated.

In the crocodile the ſucceeding or ſecondary teeth appear even when the animal's head is equal to two feet; that is, when it has acquired one-third of its uſual growth. When they grow too faſt, before the temporary tooth is ſhed, they perforate the ſide of the bone, at the part where they meet with the leaſt reſiſtance. Inſtances of this variety occur in the large crocodile's head, which is in my collection.

In all quadrupeds the enamel is, of the ſolid parts of the teeth, the firſt formed, making a cavity, in which the other bony ſubſtance is depoſited, and formed by lamellæ placed one within another, as is obſerved by Mr. JOHN HUNTER in the work already mentioned, p. 92. To this the root is added, which is filled in the ſame manner till the tooth is long enough to pierce through the gums.

But in the foſſil jaw-bones of St. Peter's mountain, a ſmall ſecondary tooth is formed, with its enamel and ſolid root at once, within the bony ſubſtance of the primordial or temporary tooth itſelf, as is to be ſeen in the ſmall fragment now in the Britiſh Muſeum, and in tab. XVI. A, B, C, D, E; which, by continuing to grow, ſeem to make by degrees ſufficient cavities in the bony roots of the primary teeth: but what becomes of them at laſt, and how they are ſhed, I am not able to gueſs. I have one in my collection, where the ſucceeding tooth is intirely formed within the center and ſubſtance of the primordial tooth. In the 6th figure (tab. XV.) a little oval cavity is obſervable, which has been the ſeat of a new or ſecondary tooth.

§ 6. The maxilla inferior of the incognitum, ſent by me to the Britiſh Muſeum, is a moſt magnificent ſpecimen, having fourteen teeth. A ſimilar one, ſomewhat longer (as it meaſures 3¼ feet) in my own collection, has alſo fourteen. Another fragment of the left ſide, two feet long and eight inches broad, ſhews the primordial and ſucceeding teeth in the cleareſt manner.

The ſpecimen, of which I ſent a drawing (tab. XVI.) to the illuſtrious Preſident of our Society, Sir Joseph Banks, is ſtill more uſeful to confirm the mode of dentition than any other I have in my muſeum.

§ 7. Several ribs and the phalanges of the toes of the fore-feet, a ſpecimen of which I ſent in a fragment from the ſame rock, of about a foot long and eight inches broad, may ſerve as another proof of the difference between theſe and the crocodile's toes, when compared with the ſtill valuable, though neglected, ſkeleton in the Britiſh Muſeum; which I am ſorry I could not make a drawing of, having been too much employed on other objects.

All theſe characteriſtic differences cannot fail to convince the learned Society of the truth of what I have aſſerted, about the animal theſe bones belonged to; for though we cannot determine exactly the ſpecies itſelf, yet I flatter myſelf the preceding obſervations evidently prove, that they did not belong to any animal of the crocodile kind.

§ 8. Another very beautiful ſpecimen, a foot and a half long, and about ten inches broad, I have been induced to add, becauſe it contains the anterior part of the ſcutum of a very large turtle. Of this Mr. John Hunter has an analogous bone from the ſame mountain in his valuable collection, but ſent to him under another name. I am convinced it belonged formerly to a turtle; firſt, becauſe I have from the ſame mountain the intire back of a turtle, four feet long and ſixteen inches broad, a little damaged at the ſides, and a pretty large fragment of another turtle, in my poſſeſſion. 2dly, Becauſe I have a ſimilar one, but ſo placed within the matrix as to ſhew the inſide, which is perfectly analogous to the inſide of that piece in the back of a large turtle I got in London, by the favour of Mr. SHELDON. 3dly, Becauſe I have amongſt theſe bones the lower jaw-bone of a very large turtle, of which the crura, though not intire, are ſeven inches long, and diſtant from one another ſix inches; the thickneſs is equal to 1¼ inch.

All theſe fragments prove the frequency of turtle bones amongſt the other foſſil bones found in the mountain near Maeſtricht.

Dr. MICHAELIS wrote to me ſome time ago, that the abovementioned fragment, in Mr. J. HUNTER's Collection, belonged to a bird; which I could hardly believe, as I never had ſeen in any collection whatſoever, either in London, Paris, Bruſſels, Gottingen, Caſſel, Brunſwic, Hanover, or Berlin, nor in my own country, any foſſil bone belonging to a bird. I know there is a ſmall one deſcribed in the Abbé ROZIER's Journal de Phyſique, for March 1782, which is at preſent in the collection of M. D'ARCET, at Paris. I expect alſo from Montmartre a ſmall leg of a petrified bird; but theſe are the only ones I have ever heard of, thoſe of Stonefield, near Woodſtock, being moſt undoubtedly of fiſhes. I think it is a circumſtance worthy the attention of the curious, that no human bones, and of birds but very few, have been hitherto found in a petrified ſtate, and belonging to the old world.

PETRUS CAMPER.

Klein Lankum, near Franeker,
June 18, 1786.

EXPLANATION OF THE PLATES.

TAB. XV.

Fig. 1, 2. Are vertebræ taken from the ſkeleton of the crocodile deſcribed by Dr. NEH. GREW, in his Catalogue of the Natural Rarities at Greſham College, p. 42. and p. 43.

a, *b*. *c*, *f*. *δ*, *ζ*. the bodies of the vertebræ; *a*, *b* of the fourth; *c*, *f* of the firſt vertebra of the neck; *β*, *z*, *t*. and *x*, *y*, *w*. the ſpinous proceſſes; *γ β*. and *s*. the aſcending; *t*, and *u v*. the deſcending proceſſes; *g*, *h*, *c*, *i*. *d*, *e*, *n*, *p*, *o*, *q*. the tranſverſe, united by cartilages to the bodies of the vertebræ. GREW calls them *oſſa mucronata*. The tranverſe proceſſes of the fourth vertebra being loſt, the roots of the mucronated proceſſes are very evident at *g h*, *i k*.

On the under part of theſe vertebræ are (*l* and *m*) proceſſes, ſimilar to thoſe we find in the vertebræ of the neck in turtles and birds. Not only the ſix poſterior but the five anterior vertebræ of the back are provided with ſuch proceſſes; of theſe, however, Dr. GREW makes no mention.

Fig. 2. Repreſents the ſeventh vertebra of the back; A, and C. are the aſcending and deſcending proceſſes, forming the articulations with the adjacent vertebræ; B. the tranſverſe proceſs, to which is united the rib FB. in B.; DE. the ſpinous proceſs; H, H, I. the body of the ſame vertebra.

Theſe figures are as large as the life, and made from the ſame ſkeleton, now in the Britiſh Muſeum. The whole length is equal to 12½ feet, Paris meaſure; the head equal to 2 feet; the neck equal to 1 foot; the trunk equal to 3 feet 8 inches; the tail equal to 5 feet 8 inches. The meaſurement given by Dr. GREW does not agree with mine; but he ſeems not to have taken it with great attention (p. 42.), for he makes uſe of the words *about*, *almoſt*, &c.

OBSERVATION. What ſtruck me was, the tranſverſe ſuture, *a*, *b*. *c*, *f*. *δ*, *ζ*. which divided the bodies of all the vertebræ of the neck, back, and loins. This diviſion ended with the os ſacrum, which was intire, as were alſo the vertebræ of the tail. Dr. GREW ſeems only to haven taken notice of the ſutures belonging to the tranſverſe proceſſes.

I have a ſmall ſkeleton of a crocodile equal to 13 inches, in which the 7 vertebræ of the neck, 12 of the back, and the 5 of the loins, are divided in the ſame manner as in the large ſkeleton in the Britiſh Muſeum. Thoſe of the os ſacrum and tail are without, and have no mark of an epiphyſis.

CONCLUSION. The tranſverſe diviſion of the vertebræ abovementioned is alſo peculiar to this animal; and there is no epiphyſis, as in other animals.

To be ſure of this, I diſſected and made a ſkeleton of the Lacerta Iguana, LINN. ſp. 26. perfectly well deſcribed by

Marcgraf, *Hiſt. Braſ.* p. 236. cap. 11.; but I found no ſuch diviſions, though the animal was young, and though it had ſtill epiphyſes on the legs, &c. The neck conſiſts of 4 vertebræ, the back of 11, the loins of 9, the os ſacrum of 2, as in the crocodile; the tail of more than 60.

The diſſection of tortoiſes ſeemed to me of conſequence, at leaſt a more accurate inſpection of the vertebræ, particularly thoſe of the neck, as being analogous in ſome reſpects to thoſe of the crocodile, eſpecially in the ſtructure of the inferior proceſſes D, and E, with *l*, *m*, fig. 1.

Fig. 3. Repreſents two vertebræ of the neck of a pretty large turtle, natural ſize.

AB, BC. the bodies; L. and I. the aſcending, H. and T. the deſcending proceſſes; R. K. the ſpinous, *a*, *b*. *d*, *e*. the tranſverſe, and D. E. the inferior proceſſes.

a, *b*, *c*. *d*, *e*, *f*. the tranſverſe diviſion of theſe, ſimilar to that in the crocodile.

Fig. 4. A vertebra from the tail of a young phocæna or porpoiſe; in which *a*, *b*. is an orbicular plate, united by means of cartilage to the body of the vertebra *a*, *d*. which is provided with ſuch a one on both ſides, *a*, *b*. and *c*, *d*.

Thoſe bony lamellæ are the epiphyſes of the vertebræ, and are alike in all quadrupeds, to which claſs all the cetaceous fiſhes belong. When we conſider the ſtructure in general of theſe laſt, we find the hind legs only are wanting, and of courſe the oſſa innominata; but the oſſa pubis are very remarkable in all of them.

Fig. 5. Is a foſſil vertebra of the unknown animal, whoſe bones are ſo often met with in St. Peter's Mountain at Maeſtricht,

tricht. A, B, C, D. is the body; C, I, K, E, F. the ſpinous proceſſes; C, K, I. the medullary canal, running under K, E, F, in a direction parallel to IF, and coming out again at F. The remaining marks of the lamellated epiphyſes I, D. and A, B. are evident proofs of the analogy between theſe and the vertebræ of the cetaceous fiſhes; and alſo of their want of reſemblance to the vertebræ of the crocodile, as will appear by comparing the firſt and ſecond figures with the fifth.

Fig. 6. Is a very accurate drawing of one of the foſſil teeth belonging to the ſame incognitum. A B C. is its point, of a lanceolated figure, whoſe edges B A, and A C, are dentated; B C. is the root, uneven, bony, fixed within the ſocket with D, G, F.; D, G, B, C. is covered with the gums; H, I. is an oval ſinuoſity, in which generally the ſecondary teeth are generated, as is ſeen in tab. XVI. repreſenting a fragment of the upper jaw-bone of the ſame incognitum, A, B, C, D, E.

The teeth in all the Phyſeteres and Delphini have ſolid roots, except in the young ones, in which they often have cavities to receive the blood-veſſels and nerves. But the crocodile has the teeth intirely hollow, as appears in

Fig. 7. in which the cavity Π, Δ, Θ, ſhews the difference between the crocodile's teeth and thoſe of the cetaceous and other fiſhes. This tooth is the anterior one of a large head of a crocodile, two feet long, and of the ſame ſize as that in the Britiſh Muſeum. A hollow tooth may notwithſtanding belong to a Phyſeter, as Dr. Otho Fabricius obſerves in his Fauna Groenlandica, p. 44. when ſpeaking of the Phyſeter microps: of which he ſays, "Habet in maxilla inferiori dentes

22, utrinque 11 arcuatos, falciformes, *intus ad apicem uſque cavos,*" within they are hollow to the very end.

T A B. XVI.

Fragmentum Maxillæ ſuperioris, lateris dextri capitis Phyſeteris incogniti, ex Monte St. Petri, Traj. ad Moſam. Origo dentium ſerotinorum ex ipſis radicibus ſolidis primo enatorum in quinque manifeſta eſt. Quæ ad dentitionem hanc ſingularem pertinent, ex figur. 2. Tab. Fragm. ſimilis ſed Maxill. inf. 12 Aug. 1784. peti debent.

Crocodili Nilotici

Colli Vert. IV.

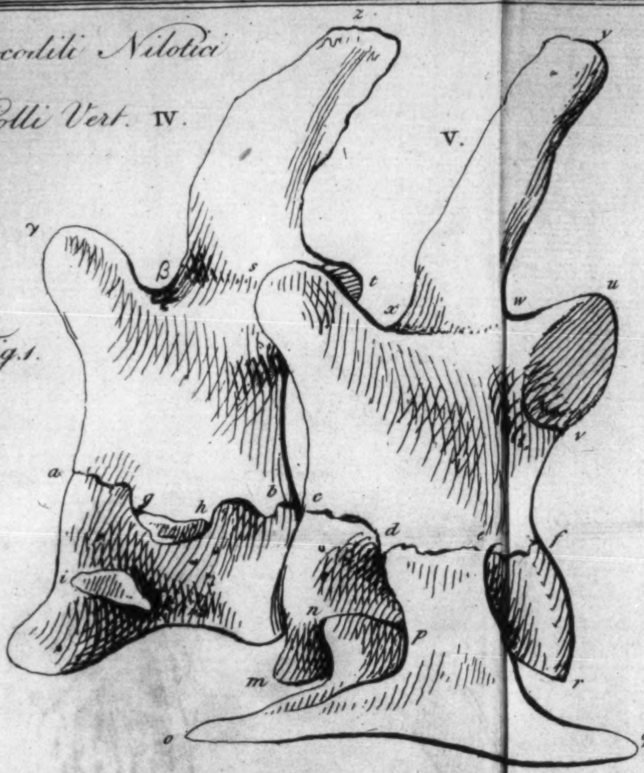
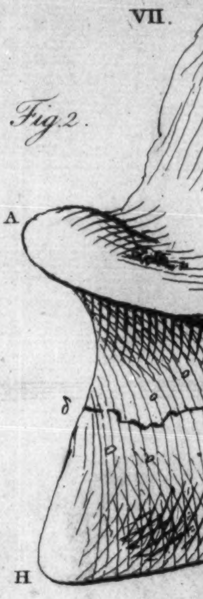


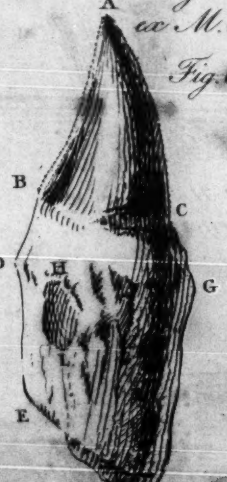
Fig. 1.

Fig. 2.



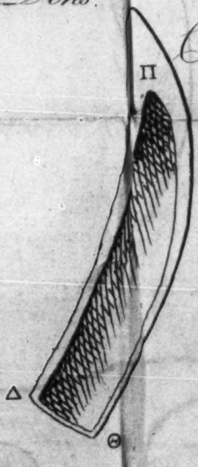
Physet. Incogn. Dens ex M. S. Petri

Fig. 6.



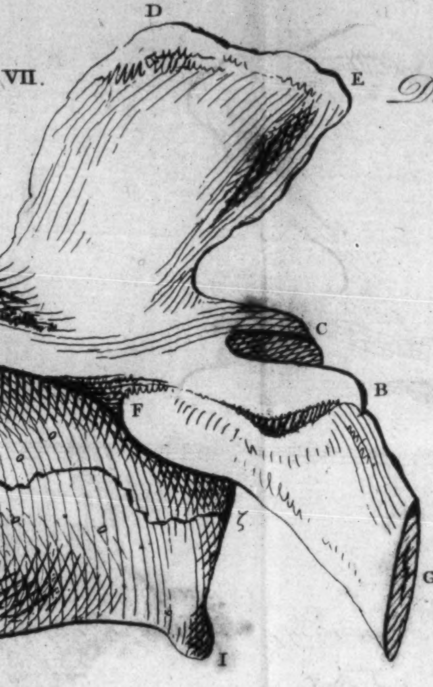
Crocodili Dens.

Fig. 7.



Verteb

P. Camper f. Lancumi, 15 Junii, 1786.



Dorsi

Testudinis Marinae
Vertebrae e Collo.
Fig. 3.

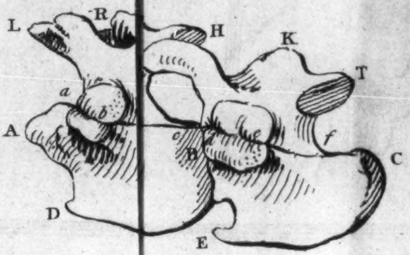
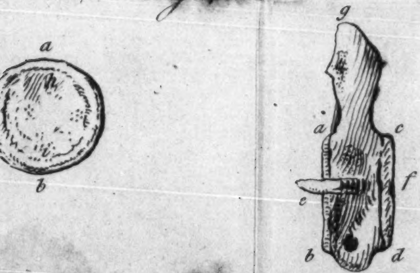


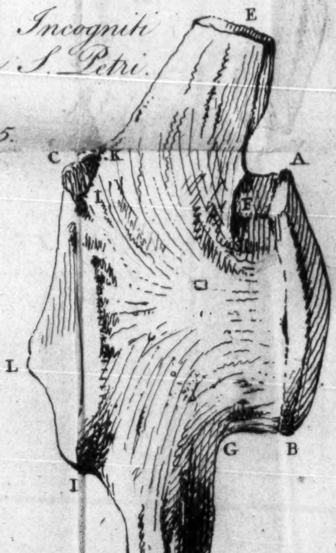
Fig. 4.



rtebra e cauda Phocaenae
Junioris

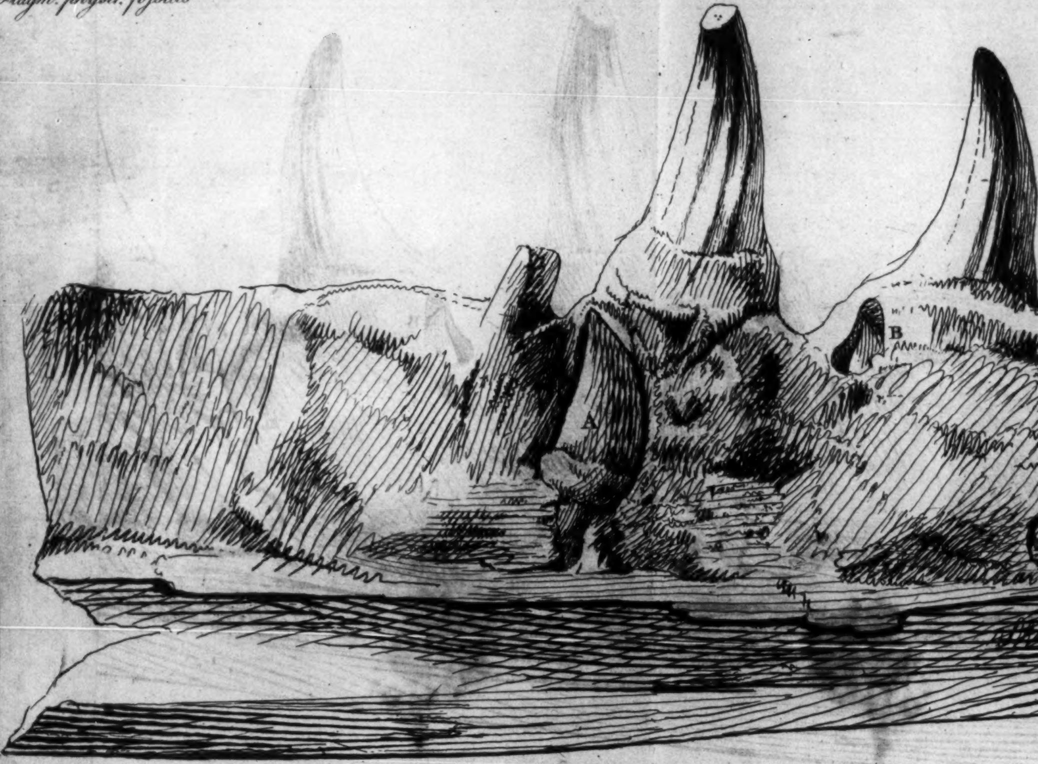
Physeteris Incogniti
ex Monte S. Petri.

Fig 5.

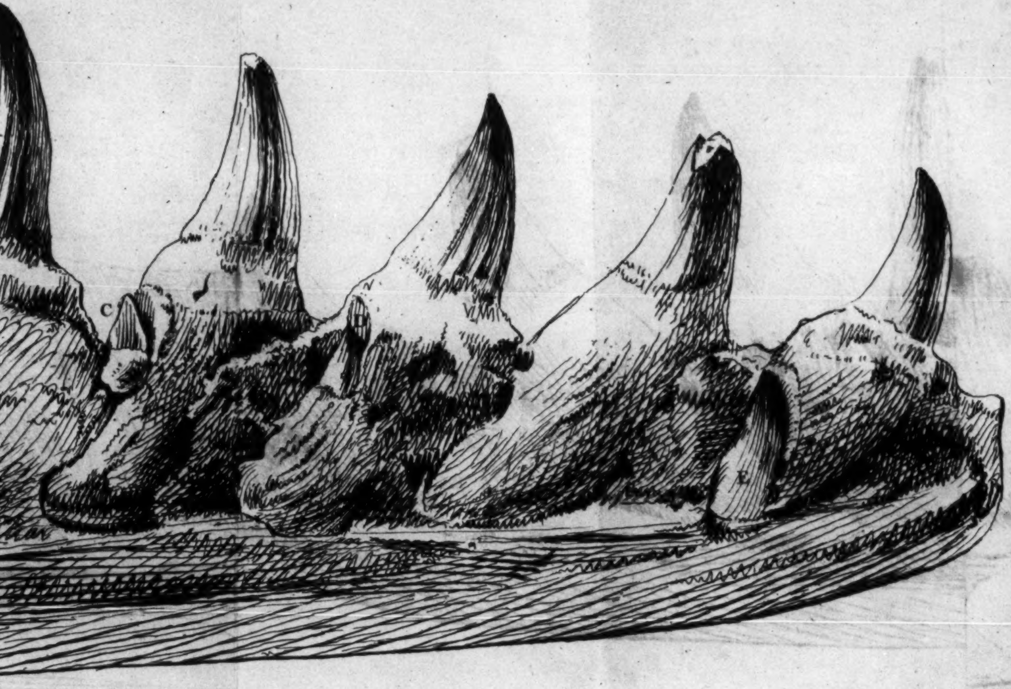


Basire S.

Fragm. physet. foſsilis



Ex Museo Petri Camper



P. Camper f.
Lancumi 27 Maji
1785.
magnit. natur.

Basire S.

XXVII. *Catalogue of One Thouſand new Nebulæ and Cluſters of Stars.* *By* William Herſchel, *LL.D. F. R. S.*

Read April 27, 1786.

THE following Catalogue, which contains one thouſand new Nebulæ and Cluſters of ſtars, is extracted from a ſeries of obſervations (or Sweeps of the heavens), which was begun in the year 1783, and which I am ſtill continuing till the whole be completed. As I may, perhaps, find an opportunity hereafter to publiſh theſe obſervations at full length, I ſhall now only mention ſuch circumſtances, relating to the inſtrument and apparatus with which they were made, as will be neceſſary to ſhew what degree of accuracy may be expected in the determination of the places of theſe Nebulæ and Cluſters of ſtars; and alſo to ſerve any aſtronomer, who wiſhes to review them, to form a judgment what inſtrument will ſuffice for this purpoſe.

The teleſcope I have uſed, as has been obſerved on a former occaſion *, is a Newtonian reflector of 20-feet focal length, and $18\frac{7}{10}$ inches aperture. The ſweeping power has been 157, except where another is expreſsly mentioned. The field of view 15′ 4″.

My eye-glaſs is mounted on that ſide of an octagon tube, which, in the horizontal poſition of the inſtrument, makes an angle of 45° with the vertical; having found, by experience, that this poſition, reſembling the ſituation of a reading deſk, is preferable to the perpendicular one commonly uſed in the Newtonian conſtruction.

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* Philoſophical Tranſactions, vol. LXXIV. p. 437.

In the preſent improved ſtate of the apparatus this teleſcope will, in general, give the relative place of an object by a ſingle obſervation true to within 1½ or 2 minutes of polar diſtance, and 4 or 6 ſeconds of time in right aſcenſion. But when there is an opportunity of repeating the obſervation, it will hardly differ a ſingle minute in the former, and ſeldom ſo much as 3 or 4″ in the latter. My apparatus, however, has not been equally perfect from the beginning; for, being from time to time adapted to the different views I had in ſweeping, it could only arrive to its preſent degree of perfection by many experiments and gradual improvements.

To begin a ſhort hiſtory of this 20-feet teleſcope. In the month of October of the already mentioned year I began to uſe it, being then mounted on its preſent ſtand, but with a lateral motion under the point of ſupport of the great ſpeculum, by which its direction could be changed about 15 degrees. It had alſo a kind of moveable gallery in front, about nine feet long, which permitted me to follow a celeſtial object near 15 degrees more; by which means I obtained a range of 30 degrees without moving the ſtand. The Newtonian form has the capital advantage of rendering obſervations equally commodious in all altitudes; I had therefore placed the inſtrument in the meridian, that I might view the ſtars in their moſt favourable ſituation.

When I had ſeen moſt of the objects I wiſhed to examine, I proceeded to the work of a general review of the heavens. The firſt method that occurred was, to ſuffer the teleſcope to hang freely in the center; then, walking backwards and forwards on the moveable gallery, I drew the inſtrument from that poſition by a handle faſtened to a place near the eye-glaſs, ſo as to make it follow me, and perform a kind of very ſlow oſcillations of 12 or 14 degrees in breadth, each taking up generally from 4 to 5 minutes of time. At the end of each oſcillation I made a ſhort memorandum of the objects I chanced to ſee; and when a new nebula or cluſter of ſtars came in my way, I made a delineation of the ſtars in the field of view, both of the finder and of the teleſcope, that it might ſerve me to find them again. This being done, the inſtrument was, by means of a fine motion under my hands, either lowered or raiſed about 8 or 10 minutes, and another oſcillation was then performed like the firſt. Thus I continued generally for about 10, 20, or 30 oſcillations, according as circumſtances would permit; and the whole of it was then called a *Sweep*, and as ſuch numbered and regiſtered in my journal.

When I had completed 41 Sweeps, the diſadvantages of this method were too evident to proceed any longer. By going into the light ſo often as was neceſſary to write down my obſervations, the eye could never return ſoon enough to that full dilatation of the iris which is abſolutely required for delicate obſervations. The difficulty alſo of keeping a proper memorandum of the parts of the heavens which had been examined in ſo irregular a manner, intermixed with many ſhort and long ſtops while I was writing, as well as the fatigue attending the motion, upon a not very convenient gallery, with a teleſcope in my hands of no little weight, eſpecially at the extremes of the oſcillations, where it made a conſiderable arch upwards, were ſufficient motives to induce me to look out for another method of ſweeping. And it is evident, that the places of nebulæ hitherto determined, which was till the 13th of December, 1783, muſt be liable to great inaccuracy. I therefore began now to ſweep with a vertical motion; and as this increaſed the labour of continually elevating and depreſſing the teleſcope by hand, I called in the aſſiſtance of a workman to do that part of the buſineſs, by which means I could obſerve very commodiouſly, and for a much longer time than before.

Soon after I removed alſo the only then remaining obſtacle to ſeeing well, by having recourſe to an aſſiſtant, whoſe care it was to write down, and at the ſame time loudly to repeat after me, every thing I required to be written down. In this manner all the deſcriptions of nebulæ and other obſervations were recorded; by which I obtained the ſingular advantage that the deſcriptions were actually writing and repeating to me while I had the object before my eye, and could at pleaſure correct them, whenever they diſagreed with the picture before me without looking from it.

In about half a dozen ſweeps, done according to this new way, I found that the ſtars of Flamsteed's Catalogue entered nearly at the time when they were expected; this ſuggeſted the poſſibility of converting my teleſcope into a tranſit inſtrument. By way of trial, Dec. 18, 1783, I began to uſe a watch, and noted the times of the tranſits of ſtars and nebulæ to the neareſt minute; and, this ſucceeding, Dec. 24, a ſidereal time-piece was introduced.

I found alſo that, by the turns of the handle which gave motion to the teleſcope, it was practicable, in a coarſe way, to aſcertain the difference of altitude between any two objects that paſſed the field of view; on which account, Dec. 30, I began to uſe an index-board, divided into inches, and marked with numbers, which, being placed behind the rope that moved the teleſcope, would point out at what altitude a certain index, affixed to the rope, was ſituated. My tackle of ropes and pullies was ſuch that, while the teleſcope traverſed an arch of two degrees, the mark on the rope paſſed over about 24 inches of the index-board: but the exact meaſure was always to be determined experimentally, as it varied according to the ſituation of the inſtrument. I perceived immediately that the quantity of rope uſed in the motion of the teleſcope would be much better obſerved by the aſſiſtant, if the index were brought within doors near the writing deſk: to effect this, I uſed a ſmall cord, which, being led off from the great one, was carried over a pulley into the obſervatory, ſo as to paſs over a ſet of numbers, which I now divided into ſuch parts as, in an equatorial ſituation of the inſtrument, would give nearly each equal to one minute.

It would exceed the limits of this Paper to enumerate the various trials I made to bring the right aſcenſion to greater perfection; ſuch as cauſing the tube ſometimes to hang inclining or rubbing againſt a perpendicular plane; at others, drawing it againſt the ſame by a ſmall weight, faſtened to a cord, paſſing over a ſide pulley, &c. I ſhall alſo paſs over the ſeveral changes in the form of the machine ſhewing the polar diſtance, which, for convenience ſake, was ſoon brought to an index moving over a dial, in the manner of a clock.

By way of directing the perſon who gives motion to the teleſcope, a ſmall machinery was added, which ſtrikes a bell at each extreme of the breadth of the ſweep, and is adjuſtable to any required number of turns of the handle.

In June, 1784, I introduced a ſmall quadrant of altitude, the uſe of which became ſoon after of the greateſt conſequence in determining the value of the numbers of the polar diſtance piece. Hitherto I had ſettled this value by cauſing a ſtar to paſs vertically through the field of the finder, which was very accurately limited to two degrees; but now I found, by many compariſons between the degree determined by the quadrant and by the finder, that I had generally under-rated the value of the numbers. Fortunately ſo many ſtars of Flamsteed's Catalogue had been taken, that the numbers between their different polar diſtances were ſufficient to recover the value of the degree; but this occaſioned a laborious re-calculation of the places of all objects taken in near 300 ſweeps. The quadrant being once introduced, I carried the refinements of the determination, in high ſweeps where the ropes acted very unequally, ſo far as to aſcertain by it ſeparately the value of every 20 or 30 minutes throughout the whole breadth of a ſweep of two degrees, and the numbers were then accordingly caſt up by ſo many different tables calculated on purpoſe.

Being ſtill diſappointed in many inſtances, when, on a review of a nebula whoſe place I had before determined, I perceived a difference of 4 or 5 minutes in polar diſtance, I began at laſt intirely to new model the machinery of the polar diſtance piece, and on Sept. 24, 1785, completed one with the following capital improvements. My former piece ſhewed a ſet of numbers whoſe value differed in every ſituation of the teleſcope, and therefore required different and very extenſive tables to caſt them up in degrees and minutes. This ſhews at once both the degree and minute of the polar diſtance of every celeſtial object, without requiring any tables to caſt up numbers. In the next place, the conſiderable inaccuracy ariſing from the unequal tenſion of the great ropes, and their expanſion or contraction by moiſture or dryneſs, is intirely taken away; for now my index cord is contrived ſo as to go off from the front of the teleſcope itſelf, in the direction of a tangent to the arch it deſcribes when moving; by which means this cord will even ſerve as an hygrometer to ſhew the variations of the ropes that ſuſpend the teleſcope. If a ſhower of rain, for inſtance, ſhould ſhorten them ſo as to elevate the teleſcope 2, 4, or 6 minutes, which has happened ſometimes, notwithſtanding they have all been well ſaturated with oil, the index cord will immediately make the polar-diſtance-clock ſhew this effect of the rain, by pointing out an equal change on the dial. As to the variations of the cord itſelf, they are in the firſt place very trifling, ſince it conſiſts merely of a few threads of hemp, very looſely twiſted, well oiled, and always equally ſtretched; but eſpecially theſe variations are of no conſequence, as they are ſo eaſily to be diſcovered by the check of the quadrant of altitude affixed to the teleſcope, or the ſucceſſive tranſits of known ſtars, and may either be immediately corrected by the adjuſtable hand of the polar diſtance dial, or be left to be accounted for afterwards.

The improvement of the right aſcenſion has not been leſs attended to; and the Royal Society having kindly intruſted me with an excellent time-piece, I ſucceeded at laſt by means of the addition of the following apparatus. Againſt the ſide of the tube is fixed a vertical iron plate, and the point of ſuſpenſion of the teleſcope is diſpoſed ſo as to permit this plate to be juſt in contact with a roller which remains fixed during the time of a ſweep. There is alſo a conſiderable ſpring applied on the oppoſite ſide, in ſuch a manner as, by always exerting a preſſure nearly uniform, to cauſe the iron plate to rub againſt the fixed roller as the teleſcope ſweeps up and down. By this means I have frequently, in very ſtormy weather, obſerved many hours without finding my time materially affected, and the corrections will ſeldom, in accurate obſervations, exceed a few ſeconds.

To

To thoſe who are accuſtomed to the accuracy of tranſit inſtruments in regular obſervatories, this teleſcope, notwithſtanding the above-mentioned improvements, may perhaps appear far from being brought to perfection; but they ſhould recollect the ſize of the inſtrument as well as its extenſive uſe, ſince I can not only follow any object for near a quarter of an hour, without diſturbing the ſituation of the apparatus, but can at pleaſure, in a few minutes, turn it to any part of the heavens, and view a celeſtial object whereſoever it may chance to be ſituated, even the zenith not excepted.

From this account it will be underſtood, that the places of a few of the nebulæ and cluſters of ſtars, determined before the 13th of December, 1783, may be faulty in right aſcenſion as far as 1′ of time, and in polar diſtance to 8 or 10′ of ſpace. Afterwards the errors will be found to become gradually leſs conſiderable till the latter end of the year 1784, when, I ſuppoſe, they will ſeldom exceed half that quantity. From that period to Sept. 24, 1785, they will diminiſh, and probably not often amount to ſo much as 3 or 4′ in polar diſtance, and 10 or 12″ in right aſcenſion. And now I flatter myſelf that all places, determined ſince the laſt mentioned time, will generally be true to a very ſmall quantity; ſuch as 4 or 6″ in right aſcenſion, and 1½ or 2′ in polar diſtance, and often much nearer.

Some of the nebulæ in that part of the heavens which, in a former Paper, I have called the ſtratum of Coma Berenices, are indeed ſo crowded that there was no poſſibility of taking them all in the center of the field of view, and a ſomewhat leſs degree of accuracy may therefore be expected; but having uſed myſelf by very frequent eſtimations of the parts of the field of view to judge of their value in time as well as in ſpace, I corrected

I corrected this defect at the moment of obſervation by affixing to the tranſits of theſe excentric nebulæ ſuch proper marks of *plus* or *minus* in right aſcenſion and polar diſtance as I judged would bring them to a central obſervation. A ſimilar method, well known to good aſtronomers in eſtimating their tenths of ſeconds by the proportional ſpace over which the ſtars move in their meridian paſſage, makes it unneceſſary to expatiate on the degree of accuracy that long practice enables us herein to obtain.

If, however, I had been willing to delay giving this catalogue till, by a repeated review of the heavens, the places had been more accurately determined, the work would undoubtedly have been more perfect; but whoever conſiders that it requires years to go through ſuch obſervations will perhaps think with me, that it is the beſt way to give them in their preſent ſtate, if it were but to announce the exiſtence of ſuch objects by way of inducing other aſtronomers alſo to look out for them. Another motive for not delaying this communication is to ſhew that my late endeavours to delineate the conſtruction of the heavens have been guided by a careful inſpection of them; and, probably, a catalogue which points out no leſs than one thouſand inſtances of ſuch ſyſtems as thoſe are into which I have ſhewn the heavens to be divided, will conſiderably ſupport what has been ſaid on this ſubject in my two laſt Papers.

When the diurnal motion of the earth was firſt maintained, it could not but greatly add to the reception of this opinion when the teleſcope expoſed to our view Jupiter, Mars, and Venus, revolving on their axes *; and if theſe inſtances of the ſimilar condition of other planets ſupport the doctrine of the diurnal motion, the view of ſo many ſidereal ſyſtems, ſome of which we may diſcern to be of a moſt ſurpriſing extent and grandeur, will in like manner add credit to what I have propoſed with regard to the condition of our ſituation within a ſyſtem of ſtars: for, to the inhabitants of the nebulæ of the preſent catalogue, our ſidereal ſyſtem muſt appear either as a ſmall nebulous patch; an extended ſtreak of milky light; a large reſolvable nebula; a very compreſſed cluſter of minute ſtars hardly diſcernible; or as an immenſe collection of large ſcattered ſtars of various ſizes. And either of theſe appearances will take place with them according as their own ſituation is more or leſs remote from ours.

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* To theſe may now alſo be added Saturn, on whoſe body I have, in the year 1780, ſeen ſeveral belts, with ſpots that changed their ſituation in the courſe of a few nights.

In the diſtribution of the nebulæ and cluſters of ſtars into claſſes, I have partly conſidered the convenience of other obſervers: thus, in the firſt claſs, the degree of brightneſs of the nebulæ has been the leading feature, as moſt likely to point out thoſe which their ſeveral inſtruments may give them expectation to reach. The firſt claſs, therefore, contains the brighteſt of them; the ſecond, thoſe that ſhine but with a feeble light; and in the third are placed all the very faint ones. Beſides this general diviſion, I have added a fourth and a fifth claſs, which contain nebulæ that, on different accounts, ſeemed to deſerve a more particular deſcription than I had allotted to the three former diviſions.

The cluſters of ſtars are ſorted by their apparent compreſſion, in the manner of my former Catalogues of double, treble, and multiple ſtars; ſo that the cloſeſt and richeſt cluſters take up the firſt claſs; the brighteſt, largeſt, and pretty much compreſſed ones, the ſecond; and thoſe, which conſiſt only of ſcattered and leſs collected large ſtars, are put into the laſt.

In every claſs the order of time when the nebulæ and cluſters of ſtars were diſcovered, or firſt obſerved with my 20-feet teleſcope, has been followed; and that I might deſcribe all theſe objects in as ſmall a compaſs as could well be done, I have uſed ſingle letters to expreſs whole words, an explanation of which, with an example of the manner of reading thoſe letters, is given. It ſhould be obſerved, that all eſtimations of brightneſs and ſize muſt be referred to the inſtrument with which the nebulæ and cluſters of ſtars were ſeen; the clearneſs and tranſparency of the atmoſphere, the degree of attention, and many more particular circumſtances, ſhould alſo be taken into conſideration; ſo that probably ſome of the nebulæ which I have called very bright, and very large, may only be juſt perceivable, as very ſmall faint patches, in many of our beſt common teleſcopes.

The Identity of each nebula in this catalogue has been well aſcertained by a projection on a proper map, made on purpoſe, which pointed out all other nebulæ near its place, and thus afforded the means of a rigorous examination. When, therefore, ſeveral nebulæ are found within the limits of the accuracy with which my teleſcope can diſcriminate them, in different nights, it may be concluded, that they were ſeen either at once in the ſame field of view, or otherwiſe in immediate ſucceſſion during the ſame ſweep.

In the ſame manner theſe nebulæ have been compared with thoſe that are contained in the two volumes of the *Connoiſſance des Temps*, for the years 1783 and 1784, of which none have been inſerted in this catalogue. It was indeed eaſy enough to diſtinguiſh the nebulæ of that excellent collection from thoſe of mine which in ſeveral places are very near them: The quantity of good light in my teleſcope having enabled me, even in bright moon-light nights, to ſee occaſionally ſome of the moſt feeble of the former, when the latter could not by any means be perceived.

Perhaps it will not be diſpleaſing to thoſe who may look out for ſome of the objects contained in this catalogue, to know that the pictures which were given in a former Paper, repreſenting the various ſhapes and appearances of ſeveral nebulæ, have been actually taken from nature, by Drawings made of them while I had them in view; I have therefore added a reference to theſe figures, as the deſcriptions of the originals which they repreſent occur in their order in the catalogue.

Arrangement of the columns, and explanations of the abbreviations.

The firſt column contains the claſs and number of the nebulæ.

In the ſecond are the dates when the nebulæ were firſt obſerved.

The third column contains the ſtar, or other object, by which the place has been determined.

In the fourth column the letter p or f ſhews that the nebula is either preceding or following the ſtar.

In the fifth is the time, in ſidereal minutes and ſeconds, by how much it precedes or follows the ſame ſtar.

The letter n or ſ, contained in the ſixth column, denotes that the nebula is north or ſouth of the determining ſtar.

In the ſeventh is the quantity, in degrees and minutes, by how much the nebula is more north or more ſouth than the ſame ſtar.

The eighth column contains the number of obſervations that have been made of each nebula; and it is to be noted, that

the

the determination of the place is generally taken from the laſt obſervation, on account of the more perfect ſtate of the teleſcope.

The ninth column, or remaining ſpace, contains the deſcription of the nebulæ, by means of ſingle letters, or now and then a few words added to them.

The abbreviations are to be underſtood as follows.

B.	Bright.	v.	very.
F.	Faint.	c.	conſiderably.
L.	Large.	p.	pretty.
S.	Small.	e.	extremely.

Of theſe letters I have compoſed vB. cB. pB. pF. vF. eF. vL. pL. pS. vS. eS.; all which require no farther explanation.

R.	Round.	l.	a little.
E.	Extended.	i.	irregularly.
M.	in the middle.	g.	gradually.
b.	brighter.	ſ.	ſuddenly.
m.	much.		

When theſe are joined we have iR. mE. lE. bM. gbM. ſbM. mbM. lbM. glbM. gmbM. ſmbM., and by taking in ſome of the former letters BM. vBM. cBM.; where no other remark will be neceſſary than that writing for inſtance bM, or brighter in the middle, it is intended to expreſs, that a nebula, which is faint at the borders, is leſs ſo towards the middle. And theſe degrees of brightneſs happening ſometimes to be ſo well united from the moſt imperceptible border to a very luminous center, I have, on ſuch occaſions, uſed the expreſſion vgmbM, or very gradually much brighter in the middle.

r.	reſolvable.	m.	milky.

er. (joined) eaſily reſolvable.

iF. (joined) of an irregular figure.

C. Cometic, or reſembling a teleſcopic comet.

N. having a Nucleus, or bright compreſſed ſpot.

l, b, or d. (joined to minutes) long, broad, or diameter.

ſt. a ſtar. ſtars.

n. north. north of.

ſ. ſouth. ſouth of.

p. preceding. np. north preceding. ſp. ſouth preceding.

f. following. nf. north following. ſf. ſouth following.

betw. between. ver. 240. verified by a power of 240.

bran. branches.

che. chevelure.

mer. in the direction of the meridian.

par. in the direction of the parallel of declination.

np ſf. in a direction from north preceding to ſouth following.

ſp nf. in a direction from ſouth preceding to north following.

Example. I. 13. 22. 69 Leon. p. 7. 57. n. 0. 2. 3. vB. mE. mer. ſmbM 7 or 8′ l.

13th nebula of the 1ſt claſs. Feb. 22, 1784. It precedes the 69th Leonis of FLAMSTEED's Catalogue 7′ 57″ in time, and is 0° 2′ more north than that ſtar. 3 obſervations. Very bright, much extended in the direction of the meridian of the nebula, ſuddenly much brighter in the middle 7 or 8′ in length.

I. 32. . . . . p. 5. 11. n. 0. 28. 3. cB. S. BN. and 2vF bran. 32d nebula of the firſt claſs. April 13, 1784. It precedes the 31ſt (or 1ſt d) Virginis of FL. Cat. 5′ 11″ in time, and is 0° 28′ more north than that ſtar. 3 obſervations. Conſiderably bright, ſmall, having a bright nucleus, and two very faint branches.

Firſt claſs. Bright nebulæ.

I.	1783	Stars.		M.	S.		D.M.		Ob.	Deſcription.
1	Dec. 19	82 (δ) Ceti	f	2	17	n	0	8	7	cB. cL. iF. bM.
2	—	3 Leonis	p	18	7	ſ	1	12	5	cB. cL. vgbM. N. R.
3	—	34 Sextant	p	28	55	ſ	0	13	4	cB. pL. C. mbM.
4	—	– –	p	28	27	ſ	0	10	4	cB. pL. C. mbM.
5	30	81 Leonis	p	2	42	n	0	7	2	B. pS. iR. bM. r.
	1784									
6	Jan. 19	64 Virginis	f	33	56	ſ	0	1	3	vB. pL. gmbM.
7	23	49 Leonis	f	126	45	ſ	0	40	1	vB. L. R. The place inac.
8	—	32 (2 *i*) Virg	f	2	50	n	0	48	5	cB. pL. iR. mbM. r.
9	24	10 (*r*) Virg	f	3	12	ſ	0	35	4	cB. E. np ſf. N and 2 bran. 3′l.
10	—	– –	f	33	37	n	0	4	4	vB. pL. lE. gmbM. 2′l. 1½′b.
11	Feb. 15	5 Comæ Be.	p	1	30	ſ	2	11	1	B. pL lE. bM. m.
12	19	6 Comæ	f	9	12	ſ	0	9	2	B. pS. R. BM. r.
13	22	69 Leonis	p	7	57	n	0	2	3	vB. mE. mer. ſmbM. 7 or 8′l. Fig. 11.
14	—	29 (γ) Virg	f	0	43	n	1	23	2	cB. cL. mE. near par. 3 or 4′l.
15	—	– –	f	3	23	n	0	58	2	cB. mE. ſp nf. ſbM. 4 or 5′ l.
16	—	– –	f	10	34	n	0	13	2	cB. vL. iF. vgmbM.
17	Mar. 11	46 (*i*) Leo	f	15	50	ſ	1	32	5	The 2 p of 3. Both vB. cL. mbM. C. II. 41. Fig. 4.
18			f	16	18	ſ	1	29	5	
19	14	11 Comæ	p	10	30	n	0	46	1	vB. pL. gbM.
20	15	73 (*n*) Leonis	f	8	52	ſ	1	57	2	vB. mE. nearly par.
21	—	– –	f	25	31	ſ	1	49	3	vB. cL. R. gmbM.
22	—	34 Virginis	p	22	24	ſ	0	17	2	cB. pS.
23	—	– –	p	18	24	ſ	0	19	2	B. S. mE.
24	—	30 (ρ) Virg	p	1	42	ſ	0	5	2	vB. pL. r. near 2 Bſt.
25	—	34 Virginis	f	4	45	ſ	0	40	1	B. S. in a line with 2 ſt.
26	19	52 (K) Leonis	p	3	45	f	2	9	1	cB. pL. not R. mbM.
27	Apr. 8	46 (*i*) Leonis	f	18	47	ſ	0	43	3	vB. BNM. and 2 F bran. np ff.
28	—	34 Virginis	p	19	36	n	1	8	2	One of two, at 4 or 5′. diſt. B. cL.
29	12	73 (*n*) Leonis	p	1	9	ſ	0	30	3	vB. cL. E. par. mbM.
30	13	31 (1 *d*) Virg	p	17	41	n	0	32	2	vB. cL. lE. iF.
31	—	31 (1 *d*) Virg	p	8	0	n	0	37	1	vB. E. mbM. r. betw. 2 Bſt.
32	—	– –	p	5	11	n	0	28	4	cB. S. BN and 2 vF. bran.
33	15	9 (*o*) Virgin	f	3	12	n	1	39	1	B. L. mE. mbM. r.
34	—	59 (*e*) Virgin	f	20	42	ſ	0	34	2	vB. cL. E np ſf. SBN.
35	17	34 Virginis	p	31	42	n	1	5	1	B vmE. vBM. 9 or 10′ l.
36 37	—	– –	p	11	24	n	0	20	1	Two. Both B. S. lE.
38	18	32 (2 *d*) Virg	p	11	36	n	0	0	1	B. vL. mE. mbM.

I.

I.	1784	Stars.		M.	S.		D.	M.	Ob.	Deſcription.
39	Apr. 24	51 (θ) Virg	p	21	36	ſ	0	14	1	vB. vL. ſmbM. rN.
40	—	– –	p	5	48	n	0	2	1	cL. vBSNM.
41	25	28 Virginis	f	9	24	n	1	2	1	B. L. iR. lbM.
42	—	26 (χ) Virgin	f	30	27	n	0	8	2	cB. L. iR. vgbM.
43	May 9	49 (g) Virgin	p	28	6	ſ	0	51	1	E. vBM. 5 or 6′ l.
44	21	51 (e) Ophiu	f	7	18	n	0	0	2	cB. pL. N.
45	24	43 Ophiuchi	p	6	36	n	0	4	2	B. R vgmbM.
46	—	– –	f	0	54	n	1	46	1	pB. cL. R. BM. r.
47	June 16	1 (*m*) Aquilæ	f	17	48	ſ	0	33	1	B. vL. iF. er. ſt viſible.
48	17	43 (*d*) Sagit'	p	114	6	n	1	44	1	B. L. R. gbM. er.
49	24	10 (γ) Sagit	p	2	18	n	0	23	1	B. pL. bM. r.
50	—	19 (δ) Sagit'	f	3	0	ſ	0	33	1	cL. R. vBM. m.
51	July 12	22 (λ) Sagit	f	3	12	ſ	0	13	1	cL. R. vBM. er.
52	Aug. 21	17 Delphini	f	6	0	n	2	24	4	vB. S. R. gmbM. r.
53	Sept. 5	66 (υ) Cygni	f	78	6	ſ	0	51	2	vB. cL. mE. mbM. r.
54	Oct. 5	35 (ν) Andr	f	12	44	ſ	2	50	1	B. cL. R. mbM. Place inacc.
55	19	66 Pegaſi	p	17	59	n	0	2	3	cB. mE. mer. gbM. 4′l. 2′b.
56 57	Nov. 16	4 (λ) Leonis	f	0	46	ſ	1	29	1	Two, at 1′ diſtance. Both cB. cL. appear like one mE.
58	17	19 Eridani	f	5	9	ſ	1	22	2	B. S. lE. mbM.
59	20	15 (ι) Navis	f	64	18	n	0	21	1	S. cBM. lE. m.
60	Dec. 9	19 Eridani	p	6	51	n	0	16	1	vB. S. lE. mbM.
	1785									
61	Jan. 6	6 Sextantis	p	8	42	n	0	31	2	vB. S. iF. 1′ nfcBſt.
62	10	55 (ζ) Ceti	p	0	25	n	0	37	2	cB. pL. E. bM.
63	—	80 Ceti	f	5	12	ſ	0	25	1	B. R. mbM. 1′ d.
64	—	8 (1 ρ) Erid	p	15	9	n	0	2	2	vB. pL. lE. mbM.
65	Feb. 7	31 Crateris	f	23	30	n	0	52	1	vB. pL. iR. bM. like 2 N.
66	8	12 Hydræ	f	25	2	ſ	1	7	1	B. vS. iF. mbM.
67	—	8 (η) Corvi	p	37	17	n	2	10	3	cB. pL. iF. mbM. 2 or 3′ d.
68	—	53 Virginis	p	12	40	n	1	4	1	cB. iR. mbM.
69	—	– –	p	11	4	n	1	34	1	cB. pL. iR.
70	Mar. 5	106 Virginis	f	1	2	n	0	54	1	vB. cL. iF. vgbM.
71	—	19 (δ) Libræ	p	0	3	n	1	4	2	cB. vS. b towards f ſide.
72	13	23 Leonis min	f	13	7	n	0	13	1	cB. cL. E. mbM.
73	—	13 Can. vena	p	50	17	ſ	0	22	1	vB. S.
74	—	13 Can. vena	p	43	5	ſ	1	11	1	cB. R. mbM.
75	—	– –	p	40	35	ſ	1	9	1	vB.
76	—	– –	p	38	3	ſ	0	52	1	cB. L. E.
77	—	– –	p	34	15	n	0	23	1	vB. L. broadly E. bM.
78	April 3	27 Urſæ	f	7	46	n	0	4	1	vB. cL. vſmbM.
79	—	– –	f	33	52	n	1	17	1	cB. pL. R. vgmbM.
80	—	– –	f	67	10	n	0	46	1	cB. S. i elliptical.
81	6	41 Leonis min	p	0	6	n	1	40	2	cB. cL. m. juſt p 2 ſt.

I.

I.	1785	Stars.		M.	S		D.	M.	Ob.	Deſcription.
82	Apr. 6	14 (*b*) Comæ	p	37	40	ſ	0	14	2	cB. pL. lE. mer. vgbM.
83	—	21 (*g*) Comæ	f	0	10	n	1	12	1	cB. pl, iR. mbM.
84	—	– –	f	19	34	n	0	55	1	cB. iR. ſBM. m. 7 or 8′ d.
85	10	40 Comæ	f	5	9	n	0	18	1	cB. pL.
86	11	39 Leonis min	p	13	14	n	0	59	1	cB. pL. mbM. brightneſs lE.
87	—	44 Leonis min	f	9	30	n	1	1	1	vB. vL. gbM.
88	—	– –	f	13	30	n	0	1	1	cB. cL. iR. mbM.
89	—	14 (*b*) Comæ	p	8	18	n	0	55	1	vB. S. lE.
90	—	– –	p	6	30	n	1	57	1	The np of 2 cB. pL R. II. 377.
91	—	15 (*c*) Comæ	f	1	10	n	0	19	1	vB. E. par. pBLN. and 2 bran.
92	—	– –	f	9	8	ſ	0	19	1	vB. vL. mE. np ſf. 10 or 12′ l. 4 ſt. in it.
93	—	31 Comæ	f	2	56	n	1	24	1	cB, pL.

Second claſs. Faint nebulæ.

II.	1783	Stars.		M.	S		D.	M.	Ob.	Deſcription.
1	Oct. 28	41 Aquarii	p	15	::	ſ	½	::	1	F. cL. mE. bM. er.
2	30	24 (α) Piſ. auſt	f	14	40	n	1	2	3	pB. S. iF. mbM.
3	Dec. 13	17 (1 φ) Ceti	p	9	::	n	2	::	2	F. L. mE. between 2 cBſt.
4	—	41 Ceti	f	15	13	n	0	37	6	pB. pS. R. mbM. C.
5	18	82 (δ) Ceti	p	0	5	n	0	46	8	pB. S. lE. bM.
6	—	– –	p	1	::	n	½	::	1	S. C, between 2 L and 1 S ſt.
7	19	45 Eridani	p	1	13	n	0	54	3	F. pL. iR. vlbM.
8	—	44 Eridani	f	2	11	ſ	0	41	4	Two. The firſt. F. S. r.
9			f	2	18	ſ	0	42	4	The ſecond, F. vS. r.
10	24	88 (γ) Pegaſi	p	13	38	ſ	0	23	2	F. pL. E. ſp nf. bM. r.
11	30	6 Comæ	f	1	24	n	0	24	2	F. pL. nearly R. r.
12	—	27 Comæ	p	3	15	ſ	0	9	2	pB. cL. lE. mbM. r.
	1784									
13	Jan. 18	78 (ι) Leonis	f	6	18	ſ	1	10	2	pB. pL. mbM. r.
14	—	3 (ν) Virginis	f	2	20	n	1	22	1	lE. not C.
15	—	20 Virginis	f	4	12	ſ	0	42	2	F. pL. pR.
16	23	56 Leonis	p	0	32	n	1	32	4	F. vS. nearly R.
17	—	9 (ο) Virginis	f	11	54	ſ	1	33	3	F. pl. E. followed by III. 91.
18	—	31 (1 *a*) Virg	p	12	28	n	1	1	2	F. S.
19	—	I. Claſs 7 Neb.	p	0	0	ſ	0	12	1	F. pS. R.
20	—	31 Bootis	p	118	45	ſ	0	38	2	vS.
21	—	32 (2 *d*) Virg	f	9	28	n	0	22	4	pB. pL. b towards the p ſide.
22	—	31 Bootis	p	80	15				1	F. vS.
23	24	75 Leonis	f	70	0	n	0	26	2	F. mE.
24	—	– –	f	97	0	n	0	7	2	F. pL.
25	—	78 Virginis	p	7	50	ſ	1	34	4	pB. cL. nearly R. mbM.

II.

II.	1784	Stars.		M.	S.		D.M.	Ob.	Deſcription.
26	Jan. 28	11 (s) Virg	f	18	0	n	0 45	1	pB. cL. b towards the f ſide.
27	30	31 Bootis	p	9	6	ſ	0 2	3	F. pL. R. lb not M.
28 29	Feb. 15	41 (ψ) Leonis	f	3	45	n	0 18	1	Two, about 2′ aſunder. Both F. cL. R. Fig 3.
30	—	68 (δ) Leonis	f	6	30	ſ	2 23	1	pB. r.
31	22	29 (γ) Virg	p	2	31	n	0 55	2	pB. cL. lE. par. r.
32	23	84 (τ) Leonis	p	6	30	n	0 7	1	pB. vS. bM.
33	—	– –	p	6	0	n	0 20	1	pB. pL. R. bM.
34	—	60 (σ) Virgin	p	51	23	ſ	1 27	4	F. S.
35	—	16 (c) Virgin	f	8	45	n	0 15	2	pB. mbM.
36	—	60 (σ) Virgin	p	46	5	ſ	1 29	3	F. vL. iR. bM. 6′ l 4′ b.
37	—	16 (c) Virgin	f	13	15	ſ	0 40	2	pB. E. np ſf. mbM.
38	—	35 Virginis	p	5	30	ſ	0 53	2	pB. pL. iF. r.
39	—	– –	p	3	0	ſ	0 20	1	pB. contains 2 ſtM.
40	Mar. 11	6 (*b*) Leonis	f	1	43	n	0 27	3	F. pL. lbM.
41	—	46 (*i*) Leonis	f	16	30	ſ	1 35	4	The f of 3. F. E.
42	—	78 (ι) Leonis	p	14	0	n	0 12	1	F. S.
43	12	36 (ζ) Leonis	p	3	10	ſ	0 40	2	pB. cL. iF.
44 45	—	20 Leonis	f	28	15	n	0 48	4	Two. Both. F. E. lbM. r.
46	—	68 (δ) Leonis	p	37	30	n	1 29	1	pB. S. r.
47	—	54 Leonis	p	2	26	ſ	1 49	2	pB. pL. lE. r. 3 or 4 ſt in it.
48	14	85 (l) Gemin	f	56	45	ſ	0 42	1	pB. pL. lbM. contains 1 ſt.
49	—	86 Leonis	p	15	0	ſ	0 19	1	vgbM. r.
50 51	—	– –	p	13	30	ſ	0 22	1	Of three that M. pB. cL. R. bM That to the n. S. R. bM. III. 2;
52	—	– –	p	10	0	ſ	0 5	1	pB. S. lE. bM.
53	—	81 Leonis	p	1	36	n	1 19	2	F. E. r.
54	—	85 Leonis	p	1	24	n	1 29	2	F. S. R.
55	—	11 Comæ	f	4	45	n	0 24	1	The f. of 2. r. See Note.
56	—	25 Comæ	p	8	30	n	0 1	2	pL. iR. bM. 2 or 3′ d.
57 58	15	5 (ξ) Leonis	p	7	15	n	0 18	1	Two, diſtant 1′ np ſf. The pS. lbM. r. The f. pL. lbM. r.
59	—	– –	f	26	30	ſ	0 53	1	vS. C. in a row with 2 F and 1
60	—	12 (*t*) Virg	p	5	15	n	0 10	1	F. S.
61 62	—	34 Virginis	p	26	12	ſ	0 31	2	Two. nearly par. The firſt pL. E. The ſecond F. pL. R
63	—	– –	p	24	42	ſ	0 9	2	F. pL. mE.
64	—	12 (ι) Virg	f	11	45	n	0 57	1	F. vS.
65	—	– –	f	14	45	n	0 50	1	pB. not vS.
66	—	30 (ρ) Virg	p	9	30	n	0 56	1	pB.
67	—	– –	p	8	30	n	1 6	1	pB. vS.
68	—	34 Virginis	p	10	24	ſ	0 36	2	pB.
69	—	30 (ρ) Virg	p	0	48	ſ	0 4	2	pB. pL. R. mbM. r.

II.	1784	Stars.		M.	S.		D.M.		Ob.	Deſcription.
70	Mar. 15	30 (ϱ) Virg	f	0	45	n	1	7	1	A nebula.
71	–	– –	f	2	0	n	1	0	1	S.
72	–	34 Virginis	f	0	15	ſ	0	54	1	S. lE.
73	–	– –	f	4	0	ſ	1	3	1	F. not vS.
74 75	–	– –	f	5	30	ſ	0	42	1	Two, nearly par. The p pB nearly R. The f. pB. vmE. 8 or 10′ diſtance.
76	–	20 (χ) Serp'	p	3	12	ſ	0	42	2	pB. pL. lE. gbM. r.
77	19	52 (K) Leonis	p	4	42	ſ	0	27	2	pB. pL. E. b. M, r. f. pBſt.
78	–	– –	p	0	12	ſ	0	27	2	pB. pL. r.
79	–	15 Bootis	f	18	30	ſ	0	15	1	F. L. R. lbM. r. 4 or 5′ dia^{r}
80	21	47 (δ) Cancri	f	4	45	n	0	55	1	pB. pL. E. r. 2 or 3 ſt in it.
81	–	51 (*m*) Leon	f	1	15	ſ	1	41	1	pB. pL. not R. r.
82	–	– –	f	8	15	ſ	1	35	1	F. S. lE. r. ſ. pBſt.
83	–	3 Comæ	p	0	15	ſ	0	43	1	F. pL. r.
84	–	– –	f	12	30	ſ	0	59	1	F. S. R. r. Note.
85 86	–	25 Comæ	p	13	0	ſ	0	21	1	Two. The p. pB. S. The f. F. S.
87	–	– –	p	11	15	ſ	1	33	1	S. bM. r.
88	–	– –	p	10	45	ſ	0	53	1	S. bM. r.
89	–	6 Comæ	f	11	12	n	0	31	2	S. bM. r. near Bſt.
90	–	25 Comæ	p	7	30	ſ	0	3	1	pB. bM. r. near Bſt.
91	–	– –	p	6	0	ſ	0	18	1	vS.
92	–	– –	p	5	30	ſ	0	24	1	S.
93	–	– –	p	4	0	ſ	0	51	1	F. vS.
94	–	– –	p	2	0	ſ	1	35	1	F. S.
95	–	27 Comæ	f	2	45	ſ	1	23	1	pB. vmE. nearly mer.
96	–	15 Serpentis	f	1	0	ſ	1	5	1	pB. pL. not R. bM. r.
97	–	26 Serpentis	f	2	30	n	0	35	1	pF. S. r. p. 2 pBſt.
98	23	8 Leonis	f	15	45	n	0	20	2	F. cL. iR. mbM. 4 or 5′ dia^{r}
99	April 8	52 (K) Leo	f	0	42	ſ	0	12	1	pB. S.
100	–	– –	f	13	30	n	0	42	1	F. pS. r.
101	–	– –	f	14	0	ſ	0	20	1	pB. S. mbM.
102	–	70 (θ) Leonis	f	0	48	ſ	0	38	1	F. pS. R. lbM. r.
103	–	94 (β) Leonis	p	9	12	n	0	47	1	F. S. E. r. 2 or 3 ſt viſible in it.
104	–	– –	p	3	18	ſ	0	48	2	pB. S. R. r. pLrN.
105	–	34 Virginis	p	35	6	n	1	17	2	pB. pL. R. vgmbM. r.
106	–	– –	p	33	18	n	1	28	2	F. pL.
107	–	6 Comæ	p	0	24	ſ	1	0	1	pL.
108	–	– –	p	0	6	ſ	1	2	1	mE. r.
109	–	– –	f	0	24	ſ	1	54	1	r.
110	–	– –	f	3	12	ſ	0	3	1	S. r.
111 112	–	– –	f	5	18	ſ	0	18	1	Two, about 2′ diſtant. The firſt R. r. The 2d, E. r.

II.	1784	Stars.		M.	S.		D.	M.	Ob.	Deſcription.
113	April 8	6 Comæ	f	10	54	n	0	9	1	E. r.
114	–	34 Virginis	p	16	54	n	1	28	2	F. r.
115 116	–	6 Comæ	f	14	0	ſ	1	21	1	Two. Both r.
117	–	– –	f	14	24	ſ	0	54	1	r.
118	–	– –	f	15	36	ſ	0	28	1	S. Note.
119	–	– –	f	18	54	n	0	35	1	pL. r.
120	–	– –	f	19	6	ſ	0	25	1	L. r.
121 122	12	34 Virginis	p	17	54	n	1	21	1	Two. Both pF. S. bM.
123 124	–	– –	p	16	24	n	0	29	2	The two p. of 3. Both F. S. bM. Note.
125	–	– –	p	4	18	n	1	25	1	not vF. S. r.
126	–	– –	p	3	0	n	1	11	2	pB. L. E. r.
127	–	– –	p	2	30	n	1	39	1	F. vS. R. lbM. r.
128	–	– –	f	0	42	n	1	54	1	L. R. bM. r.
129	–	41 Virginis	f	19	0	n	0	11	1	F. pL. lbM. R. r.
130	–	20 (χ) Serp^s	p	12	30	n	0	12	1	F. not S. iF. r.
131	13	56 Leonis	p	3	48	ſ	0	26	1	pB. vL. nearly R. lbM.
132	–	8 (π) Virgin	p	4	48	n	0	7	1	pL. E. pBM. r.
133	–	) (ο) Virginis	f	7	46	ſ	1	41	2	not vF. S. E. mer.
134	–	11 (*s*) Virgin	f	4	24	ſ	0	0	1	F. mE.
135	–	– –	f	5	54	n	0	38	1	S. E. pBM.
136	–	– –	f	6	30	n	1	39	1	F. S. iF. r.
137	–	9 (ο) Virginis	f	12	32	ſ	2	2	2	F. pL. r.
138	–	11 (*s*) Virgin	f	9	6	n	0	20	3	F.
139 140	–	– –	f	9	30	n	0	0	3	Two. The 1ſt is the largeſt. The 2d vF.
141 142 143	–	– –	f	13	18	n	0	18	3	Three nebulæ. The laſt is the largeſt.
144	–	31 (1 *d*) Virg	p	17	9	n	0	42	2	F. pL. the largeſt of 2.
145	–	60 (σ) Virg	p	50	28	n	0	30	2	vF. S. E.
146	–	31 (1 *d*) Virg	p	14	6	ſ	0	34	1	F. pL.
147	–	– –	p	7	36	ſ	0	20	1	pB. pL. mE. r.
148	–	– –	p	0	18	n	0	31	1	not F. R. vgbM.
149	–	– –	f	0	9	n	0	53	4	F. pL. iF. r.
150	–	24 (α) Serp^s	p	73	42	n	1	10	1	F. pL. nearly R. er.
151	–	12 Herculis	f	4	18	ſ	0	26	1	not vF. pL. iR. bM. r.
152	15	78 (ι) Leonis	f	4	24	ſ	1	1	1	F. mE. r.
153 154	–	2 (1ξ) Virgin	p	3	0	n	2	2	1	Two, about 5′ diſtant. Both F. pS. C.
155	–	20 Virginis	p	6	6	ſ	0	40	1	F. pL. lE. lb. towards p. ſide.
156	–	– –	p	4	36	ſ	0	26	1	F. pL. lE. r.

II.	1784	Stars.		M.	S.		D.M.		Ob.	Deſcription.
157	April 15	20 Virginis	p	3	36	ſ	1	29	1	F. pL. mE. bM. r.
158	—	31 (1 *d*) Virg	p	8	38	n	1	51	3	pT. pL. nearly R. r.
159	17	81 Leonis	f	0	36	n	0	24	1	pB. S. bM. almoſt ſtellar.
160	—	— —	f	1	0	n	0	45	1	cL. R. vgbM.
161	—	90 Leonis	f	5	36	n	0	53	1	F. not S. R. bM.
162	—	34 Virginis	p	51	54	n	0	0	2	not vF. pL. iR. lb. towards f. ſide.
163	—	— —	p	33	6	n	1	13	1	pS.
164	—	— —	p	32	48	n	0	13	1	pS. vmE.
165	—	— —	p	32	30	n	1	13	1	F. vmE.
166	—	— —	p	27	36	n	0	53	1	pB. vS.
167 168	—	— —	p	21	30	n	0	49	1	Two nebulæ. The moſt ſ. E.
169	—	— —	p	20	30	n	0	40	1	S.
170	—	— —	p	19	42	n	0	49	1	F.
171 172 173	—	— —	p	19	6	n	0	20	1	Three nebulæ. The two firſt vS. The third S.
174	—	— —	p	17	48	n	1	16	1	F.
175	—	— —	p	12	36	n	1	9	1	pF. L.
176	—	— —	p	3	48	ſ	0	37	1	F.
177	—	20 Bootis	f	3	30	ſ	1	42	1	pF. not S. lbM. r.
178 179	—	28 (β) Serpᵗ	p	12	6	ſ	0	7	2	Two, very cloſe. Both S. ſtellar. The ſ. is largeſt.
180	22	15 (ν) Virg	f	8	59	ſ	1	18	3	pB. L. iR. er.
181	—	29 (γ) Virg	f	5	18	ſ	0	58	1	pF. pL. E. r.
182	—	— —	f	6	24	ſ	1	54	1	pF. pL. E. r.
183	24	51 (θ) Virg	p	30	36	n	0	14	1	pB. cL. E. vſmbM.
184	—	— —	p	28	30	n	0	26	1	not F. L. lE. lbM. r.
185	—	— —	p	11	0	n	0	10	1	F. S. iF. near pBſt.
186	25	28 Virginis	f	12	6	n	0	51	1	pF. cL. R. r.
187	—	— —	f	12	42	n	0	37	1	pF. pL. r.
188	—	— —	f	22	54	n	0	57	1	F. cL. E. r.
189	—	72 (1 *l*) Virg	p	21	54	ſ	0	18	1	pB. R. vſmbM. near Bſt.
190	—	26 (χ) Virg	f	23	44	ſ	0	6	2	F. pL. iR. lbM. r.
191	May 9	49 (*g*) Virg	p	4	6	ſ	0	46	1	pF. pS. R. r. near ſome Sſt.
192	—	18 Libræ	f	10	36	ſ	0	16	2	pF. pL. lE. mer. nearly.
193	11	100 (λ) Virg	p	59	30	n	0	48	2	The moſt n. of 3. pB. vS. bM.
194	19	12 (*d*) Bootis	f	7	42	ſ	0	2	2	F. pL. R. mbM.
195	21	39 Ophiuchi	p	12	54	n	1	42	2	pB. cL. iR. lbM. r.
196	22	54 Hydræ	p	6	42	ſ	1	2	1	pB. S. nearly R. bM. r.
197	—	51 (ε) Ophiu	f	35	36	ſ	1	13	1	pB. pL. iR. r.
198	24	3 (*p*) Sagittⁱ	f	18	42	ſ	0	4	1	pF. not L. crookedly E. er.
199	June 16	64 (ν) Ophiu	f	2	48	n	0	48	1	pB. pL. R. gbM. r.
200	24	10 (γ) Sagittⁱ	p	1	6	n	0	22	1	F. pS. r. unequally B.

II.	1784	Stars		M.	S.		D.M.		Ob.	Deſcription.
201	July 13	18 Sagittarii	p	7	54	ſ	0	55	1	F. pL. lbM. r.
202	17	12 (φ) Cygni	f	17	36	ſ	0	53	1	A reſolvable nebulous patch of ſt.
203	—	65 (ζ) Cygni	p	9	30	ſ	0	16	2	pB. pL. iE. bM.
204	Aug. 7	24 Sagittarii	p	9	18	n	0	50	1	pB. S. ſtellar. not verified.
205	—	— —	p	1	42	n	0	33	1	pB. cL. iE. bM.
206	Sept. 7	52 (ι) Cygni	f	5	36	n	1	22	1	F. S. crookedly E. r.
207	—	44 (η) Pegaſi	p	34	27	n	1	15	1	cL. R. gmbM. er.
208	10	84 (ψ) Pegaſi	p	13	48	n	1	0	1	F. cL. R. vgbM. ſf. ſt.
209	—	34 (ζ) Andr	p	5	57	n	1	12	2	F. pL. iR. equally B. r.
210	11	31 (δ) Andr	f	18	12	ſ	0	26	1	F. pL. unequally B. near pBſt.
211	—	13 Triang	f	5	24		0	35	1	F. pL. lE. bM. n. 2 ſt.
212	12	63 Pegaſi	p	19	42	ſ	0	15	1	pB. pL. lE. mbM. r. ſ. 2 Fſt.
213	—	79 Pegaſi	p	2	36	n	0	42	1	F. pL. ER. lbM.
214	—	40 Androm	p	7	18	ſ	0	15	1	F. E. p. Bſt.
215	—	— —	f	4	30	n	0	41	1	Three. mer. Nearly equal in ſize. All. F. vS. R. proportion of diſt. ſ to n. 2 to 1.
216										
217										
218	—	— —	f	5	30	n	1	22	1	F.
219	—	— —	f	7	36	n	1	22	1	Two. The p. F. vS. The f. pL.
220										
221	—	3 (ι) Triang	p	6	12	ſ	0	15	1	F. pL. mE. r. 1½' l.
222	—	— —	p	5	12	ſ	10	0	1	F. pL. mE. r. 1½' l.
223	—	— —	p	2	12	ſ	1	52	1	pB. pS. R.
224	13	43 (β) Andr	p	0	18	n	0	5	1	pB*. cL. R. bM. * Though β And. in the field.
225	—	9 (γ) Trian	f	4	18	ſ	0	39	1	F. vS. R.
226	15	71 (y) Pegaſi	p	4	54	ſ	0	5	1	F. pL. bM. elliptical.
227	—	89 (χ) Pegaſi	p	10	18	n	0	32	2	F. cL. mE. r.
228	—	6 (β) Arietis	p	5	12	n	1	7	1	Two. Both F. pS. iR.
229										
230	18	81 (φ) Pegaſi	p	1	27	n	1	4	1	F. pL. R. bM. r.
231	—	— —	p	1	3	n	0	59	1	F. pL. E. par. contains a ſtell. or ſt.
232	—	— —	f	6	45	n	1	35	1	F. S. R. or large ſtellar.
233	19	47 (λ) Pegaſi	p	9	3	n	0	12	3	Two. The p. pB. lE. nearly mer.
234			p	8	33	n	0	14	3	The f. F. E. nearly par. 1'½ l.
235	20	11 Piſcium	p	13	43	ſ	0	36	2	F. pL. broadly E.
236	—	90 (φ) Aqua	f	3	53	n	1	22	4	pB. pL. iR. mbM.
237	—	79 Ceti	p	4	48	n	0	36	1	F. E. mer. 2' l.
238	Oct. 6	26 (β) Perſei	p	28	34	ſ	0	10	2	pB. mE. near par. mbM. 4' l 1' b.
239	7	27 (κ) Perſei	p	8	27	n	0	2	1	The 1ſt of 2. pB. pS. r.
240	8								1	pF. pL. iR. er.
241	—								1	pS. C.
242	11	48 (μ) Pegaſi	p	39	50	ſ	0	54	2	F. S. iR. near and p. 2 or 3 ſt.
243	—	— —	f	6	27	ſ	0	54	2	F. S. iR.

II.	1784		Stars.		M.	S.		D.	M.	Ob.	Deſcription.
244	Oct.	14	54 (α) Pegaſi	f	30	48	n	0	6	2	F. S. lE.
245		—	58 Piſcium	p	3	36	n	2	16	4	pB. pL. R. lbM.
246		—	19 Arietis	f	4	54	ſ	0	49	1	F. pL. E. 4 or 5′ ſ. cſt.
247		15	13 Pegaſi	f	10	0	n	0	28	1	pB. R. bM. 1′ d.
248		—	54 (α) Pegaſi	p	38	0	n	0	59	2	F. pS. a quartile with 3 Sſt.
249		—	– –	p	3	36	n	1	11	2	F. pS. E. f. pBſt.
250		—	47 Piſcium	p	67	12	ſ	0	37	1	F. lE. p. vBſt.
251		16	54 (α) Pegaſi	p	4	36	n	0	44	1	pB. cL. E. r.
252		—	102 (π) Piſc	p	12	48	n	0	45	1	F. pL. oval. lbM. p. pBſt.
253		—	– –	f	5	54	n	1	30	1	pB. pL. E. bM. r.
254		—	38 Arietis	f	8	48	n	0	34	1	F. S. iR. r.
255		18	82 Pegaſi	p	8	21	ſ	0	11	2	pB. pS. R. gbM. r.
256		—	77 Pegaſi	f	1	0	ſ	0	25	1	F. R. gbM.
257		—	34 Piſcium	f	12	6	ſ	0	39	2	F. pL. iR. mbM.
258		20	15 Eridani	p	8	54	n	1	54	3	F. vL. lbM. R. 7 or 8′ d.
259	Nov.	16	43 (γ) Cancri	p	20	58	n	1	2	1	F. S. iF.
260		—	4 (λ) Leonis	f	3	22	ſ	1	16	1	F. pS. lE.
261		17	12 Pegaſi	f	2	8	ſ	0	46	1	F. iR. leſs than 1′ d.
262		—	27 Eridani	p	11	51	ſ	1	40	1	F. l and iE. above 1′ d.
263		—	– –	p	9	28	ſ	1	15	1	not vF. bM. 1′½ d.
264		—	47 (δ) Cancri	p	67	42	n	2	20	1	F. S.
265		20	4 (1 χ) Can	p	19	20	n	1	28	1	pF. pS. iF. lE. bM.
266		—	15 (ι) Nav	f	25	33	n	1	25	1	F. E. bM. r. 1′½ d.
267	Dec.	9	27 Eridani	p	6	1	n	0	40	1	F. vS. R. lbM.
268		—	8 (ι) Crateris	p	63	16	ſ	0	16	1	F. S. R. SB point M. C.
269		—	10 Crateris	f	4	26	n	1	22	1	pB. pL. lE mbM.
270		13	106 (ν) Piſc	f	11	56	ſ	1	11	1	pB. S. iR. mbM.
271 272		—	– –	f	14	54	n	0	11	3	Two, very cloſe nearly par. The f ſmalleſt and moſt n.
273		—	86 (γ) Ceti	p	0	14	n	1	44	1	F. S. iR.
274		—	92 (α) Ceti	p	3	54	ſ	0	47	1	F. vS. iE. er.
275		20	32 (2 τ) Hyd	f	9	55	n	1	32	2	pB. cL. iR.
276		—	10 (r) Virgin	p	6	58	n	0	5	3	F. pL. R. lbM.
277		—	– –	p	5	14	ſ	0	1	3	F. S.
	1785										
278	Jan.	6	75 Ceti	p	1	38	ſ	0	5	1	pB. S. E.
279		—	35 Eridani	f	2	55	ſ	0	38	2	F. mE. vlbM. about 4′ l.
280		—	14 Hydræ	f	5	2	n	0	21	1	F. vS. lE. ver. 240.
281		—	28 (A) Hydr	p	29	27	n	1	40	2	F. vS. E.
282		10	41 Ceti	f	17	28	n	0	20	2	pB. cL. lE. mbM.
283		—	– –	f	21	26	n	0	10	2	pB. S. mbM.
284		—	80 Ceti	f	3	34	ſ	0	19	1	F. mE. about 3′ l and ¾′ b.
285		—	55 (ζ) Ceti	f	74	50	n	1	2	2	pB. E. ſp nf. about 1 ½ l.
286		—	– –	p	4	34	ſ	0	9	1	F. pL. R. lbM. ſ. Sſt.

II.	1785	Stars.		M.	S.		D.	M.	Ob.	Deſcription.
287	Jan. 27	17 Eridani	p	10	24	ſ	1	12	2	F. vS. lE. er. unequally B.
288	28	21 Eridani	p	1	55	n	0	35	3	F. pL. iR. r.
289	31	7 (ν) Lepor	f	2	32	n	0	51	1	F. pL. l triangular F. r.
290	Feb. 1	89 (π) Ceti	f	49	17	n	0	21	3	F. pL. R. lbM. f. pLſt.
291	—	26 (π) Erid	p	3	39	ſ	1	25	1	pF. mE. mer. 3 or 4′ l and 1′ b.
292	4	5 (μ) Lepor	p	0	50	n	0	29	1	pB. iR. mbM. ſp. pcſt.
293	7	6 (3 *b*) Crater	p	52	51	n	0	23	1	pB. S. iR. bM.
294	—	31 Crateris	p	6	45	n	0	6	1	F. S. E. r.
295	—	– –	p	1	48	n	1	18	1	F. vS. iF. bM.
296	—	– –	p	0	12	n	0	24	1	pB. pL.
297	—	89 Virginis	p	11	47	n	0	18	1	pF. L. mbM.
298	8	8 (η) Corvi	f	18	44	n	1	51	1	F. pL. lbM. ½′ p. is a S ſuſpected ſtellar.
299	—	53 Virginis	p	12	30	n	0	48	1	pB. pL. mbM.
300	—	– –	p	11	0	n	2	8	2	pF. eL.
301	—	– –	p	3	8	n	0	34	1	pB. pL. iR. mbM.
302	28	2 (1ω) Cancri	p	3	5	ſ	1	40	1	pF. vS. bM. er.
303	—	19 (λ) Cancri	p	2	22	ſ	0	35	1	F. S. mbM. r.
304	Mar. 4	11 Monoc	f	30	53	ſ	0	37	3	Some Sſt with pB nebuloſity.
305	5	20 Sextantis	p	7	14	n	0	49	1	F. S. lE. er.
306	—	88 Virginis	f	0	52	ſ	0	24	1	F. vS. iF. r.
307	—	– –	f	3	58	n	0	43	1	F. cL. iF. bM.
308	—	82 (*m*) Virg	f	12	28	n	1	6	2	F. S. iR. lbM.
309 310	—	99 (ι) Virg	p	12	31	ſ	0	1	1	Two. nearly mer. diſt. 4′ Sſt. betw. che. touch. n. pB. cL. mbM. ſ. F. S.
311	10	6 (3*b*) Crate	p	68	34	ſ	1	18	2	cB. S. mbM.
312	—	45 (ψ) Hydr	f	9	41	n	2	0	1	F. L. iR. vgbM.
313	—	– –	f	10	53	n	1	16	1	pB. lE. par. b towards f. ſide.
314	—	– –	f	17	57	n	1	55	1	F. S. iF. bM.
315	11	23 (2φ) Can	f	0	29	ſ	1	0	2	F. S. R. bM. C. N.
316 317	12	64 (1*b*) Gem	p	4	16	n	1	17	1	Two. ſp nf. diſt. 1′ che. mix. Both F. S. equal. N.
318	—	22 (1φ) Can	f	8	38	n	0	36	1	F. pL. lE. mbM. r.
319	—	48 (1ι) Canc	p	9	10	ſ	0	5	1	F. S. bM. r.
320	13	23 Leonis min	p	12	38	n	1	50	1	F. pS. R. lbM.
321	—	13 Can. ven.	p	51	31	ſ	0	50	1	pB. L. gbM.
322 323	—	– –	p	40	19	ſ	1	28	1	The two firſt of 3 in a line. of unequal ſize and brightneſs.
324	—	– –	p	38	3	n	0	17	1	F. S.
325	—	– –	p	26	51	ſ	0	30	1	F. pL. E. bM.
326	—	– –	p	14	11	ſ	0	4	1	F. mE. mer.
327	—	– –	f	19	43	ſ	0	35	1	F. pS.
328	—	– –	f	23	43	n	0	38	1	pB. pS. nearly R. mbM.

II.	1785	Stars.		M.	S.		D.M.		Ob.	Deſcription.
329	Mar. 13	49 (δ) Bootis	p	48	50	n	0	5	3	pF. S. R. r. n. 2 pBſt.
330	—	– –	p	45	45	ſ	2	2	1	pB. pL. R. bM.
331	16	11 Urſæ min	p	60	36	ſ	0	2	1	F. pS. er.
332	—	– –	p	20	10	ſ	0	2	1	pB. cL. b towards p. ſide.
333 334	April 3	27 Urſæ	f	20	14	ſ	0	2	1	Two. Nearly mer. Moſt n. pB. pS. bM. Moſt ſ. F. S. bM.
335	—	– –	f	73	0	n	1	41	1	pF. cL. iE.
336	—	– –	f	88	16	n	0	30	1	pB. vS. iR.
337	—	– –	f	94	42	n	0	48	1	pF. pS. bM.
338	6	44 Leonis min	f	31	8	ſ	0	59	2	F. cL. iR. gvlbM.
339	—	53 Leonis min	f	19	26	n	1	0	1	pF. pS. iF.
340	—	72 Leonis	f	25	8	n	1	35	2	F. vS. ſtellar. ſhort ray p. ſide.
341	—	4 Comæ	p	29	46	n	0	35	1	F. ſtellar.
342	—	– –	p	22	2	n	0	37	1	F. pL.
343	—	21 (g) Comæ	f	4	36	n	1	56	1	not L.
344	—	– –	f	20	56	n	1	11	1	F. pL. lE.
345	—	– –	f	23	34	n	2	28	1	juſt ſ. pBſt.
346	—	31 Comæ	f	4	54	ſ	0	34	2	F. pL. iF.
347	10	36 (ζ) Leonis	f	11	14	ſ	0	33	1	pB. S. bM. r.
348	—	41 Leonis min	p	3	34	n	0	51	1	F. S. lE.
349	—	72 Leonis	f	14	12	n	1	1	1	F. pL. i triangular F.
350	—	– –	f	16	2	ſ	0	17	1	F. S.
351	—	– –	f	18	2	n	1	22	1	F. S.
352	—	92 Leonis	p	3	9	n	1	16	2	F. pS.
353	—	7 (b) Comæ	p	4	37	n	0	11	1	pB. cL. iF. bM.
354	—	– –	p	0	43	n	0	2	1	F. vS.
355	—	22 Comæ	p	4	32	ſ	1	28	1	pF. L. broadly E.
356	—	40 Comæ	f	5	38	n	1	25	1	pB. S.
357	—	12 (d) Bootis	f	18	46	ſ	1	53	1	F. S. iF. lbM.
358	11	39 Leonis mi	p	8	4	n	0	8	1	F. pL.
359	—	– –	p	7	38	n	1	1	1	pB. pS. nearly R. bM.
360	—	44 Leonis mi	p	1	46	n	0	39	1	F. pL. iF.
361	—	– –	f	0	50	n	0	30	1	F.
362	—	– –	f	1	18	n	0	1	1	pB. pL.
363	—	– –	f	1	32	n	0	8	1	F. S.
364	—	– –	f	4	35	ſ	0	44	1	pF. pL. lE. b towards ſf. ſide.
365	—	– –	f	14	2	n	0	56	1	F. mE. 1½′ l. but v. narrow.
366	—	– –	f	14	24	n	0	4	1	pF. pL.
367	—	– –	f	42	12	n	0	14	1	F. vS.
368	—	14 (b) Comæ	p	28	42	n	0	59	1	pF. bM.
369	—	– –	p	27	58	n	0	12	1	F. pL. E. b towards f. ſide.
370	—	– –	p	20	16	n	0	55	1	pB. cL. mb towards nf. ſide.
371	—	– –	p	17	40	n	1	55	1	One of three. F. iF.

II.	1785	Stars.		M.	S.		D.M.		Ob.	Deſcription.
372	Apr. 11	14 (*b*) Comæ	p	74	24	n	1	55	1	One of 4. The moſt n. of the p. ſide of a quartile. F. S.
373	—	— —	p	13	28	n	1	16	1	F. L. bM.
374	—	— —	p	12	22	n	1	12	1	F. S.
375	—	— —	p	11	4	n	1	14	1	F. pS.
376	—	— —	p	6	38	n	0	22	1	pF. S. almoſt R. bM.
377	—	— —	p	6	30	n	1	57	1	About 6′ ſf I. 90. pB. S. the place is that of the np.
378	—	— —	p	4	10	n	1	57	1	F. cL. lE.
379	—	— —	p	1	36	n	1	18	1	F. S.
380	—	15 (*c*) Comæ	f	9	8	ſ	1	22	1	F. pL.
381	—	31 Comæ	p	3	46	ſ	0	20	1	F. S.
382	—	— —	f	3	16	ſ	0	9	1	F. pS.
383	—	— —	f	4	26	n	0	12	1	F. pL.
384	—	— —	f	5	2	ſ	0	23	1	F. pL.
385	—	— —	f	5	40	n	0	4	1	F. pL.
386	—	— —	f	5	54	ſ	0	6	1	F. pL.
387	—	— —	f	5	48	n	0	55	1	F. pL.
388 389	—	41 Comæ	p	7	46	n	0	22	1	Two. The time taken between them.
390	—	— —	p	7	10	ſ	0	43	1	F.
391	—	— —	p	7	18	n	0	23	1	F.
392 393 394	—	— —	p	5	46	n	0	14	1	Three. The 2 f. p near each other. The ſp. about 8′ diſt. The time is that of the 2.
395	—	— —	p	3	26	n	0	33	1	F. S.
396	—	— —	p	2	16	n	1	29	1	F. S.
397	—	— —	p	2	2	ſ	0	4	1	F. S.
398	—	— —	p	1	30	n	0	8	1	F. S.
399	—	3 (β) Coron	f	6	54	ſ	0	27	1	pF. pL. iR. bM. r.
400	13	26 Bootis	f	47	12	ſ	1	33	1	F. pL. er.
401	14	11 Serpentis	p	2	14	ſ	1	35	1	pF. pL. vlbM. r. p. 3 Sſt.
402	—	12 Ophiuchi	p	14	32	n	0	4	1	F. cL. E. ſp nf. r. 3′ l 2′ b.

Third claſs. Very faint nebulæ.

III.	1783	Stars.		M.	S.		D.M.		Ob.	Deſcription.
1	Nov. 3	36 (*υ*) Orion	f	3	39	n	1	57	2	vF. S. mE. In the L. neb.
2	Dec. 21	60 Ceti	f	13	::	n			1	eF. vS. R. lbM.
3	30	95 (*o*) Leonis	f	4	15	n	0	36	2	vF. vS. lE. r.
	1784									
4	Jan. 18	6 (*h*) Leonis	f	6	4	ſ	0	9	3	eF. vS. iE. ſp. a triangle of Bſt.
5	—	47 (*ρ*) Leonis	f	10	0	n	0	9	1	eF. eS. viewed alſo with 240.
6	—	59 (*e*) Virgin	p	28	::				1	vS.

III.	1784	Stars.		M.	S.		D.M.		Ob.	Deſcription.
7	Jan. 23	3 (β) Can. mi	f	36	30	n	0	19	1	Stellar. 240 left ſome doubt.
8	—	3 Leonis	f	1	6	ſ	0	28	3	E. er. 3 of the ſt. viſible.
9 10	—	32 (2 d) Virg	f	46	54	ſ	0	25	2	Two. Both vF. and vS.
11	—	31 Bootis	p	38	15	ſ	0	1	1	vF. ſtellar.
12	—	— —	p	21	15	ſ	0	34	1	vF. forming an arch with 3 ſt.
13	28	11 (ſ) Virg	f	27	30				1	eF. not verified.
14	30	31 Bootis	p	12	30	ſ	0	9	1	eF. vL. not verified.
15 16	Feb. 15	68 (δ) Leonis	f	7	30	ſ	0	24	1	Two. The p. vF. L. 5 or 6′ dia. The f. eF. S. Fig. 5.
17	23	16 (c) Virgin	f	6	0	ſ	0	47	1	vF. pS. r.
18	—	— —	f	11	45	n	0	38	1	vF. cL. r.
19	Mar. 11	2 (ε) Can. mi	f	5	16	n	0	28	1	2vS and cloſe ſt. with nebuloſity left doubtful.
20	—	53 (l) Leonis	f	1	45	ſ	0	26	1	vF. r.
21	—	73 (n) Leonis	p	15	36	ſ	1	11	2	vF. S. C. ver. 240.
22	—	53 (l) Leonis	f	14	0	n	0	31	1	vF. vS. with 240 cL.
23	—	73 (n) Leonis	p	9	6	ſ	1	56	2	vF. vS. lE. ver. 240.
24	12	20 Leonis	f	11	30	n	1	19	1	vS. 240 left ſome doubt.
25	—	— —	f	26	15	n	0	0	1	vF. S.
26	—	20 Comæ	f	4	30	ſ	0	37	1	eF. L. left doubtful.
27	14	86 Leonis	p	13	30	ſ	0	22	1	The moſt ſ. of 3. vF. vS. II. 50. 51.
28	—	— —	p	2	30	ſ	1	10	1	vF. L. r.
29	—	— —	f	7	15	ſ	0	34	1	vF. eS. ſtellar. ver. 240.
30	—	— —	f	10	0	ſ	0	40	1	vF. pS. f. 2 vBſt.
31	—	11 Comæ	f	3	15	ſ	0	10	1	eF. forms a triangle with 2 Sſt.
32	—	8 (η) Bootis	f	16	0	ſ	0	56	1	vS. or nebulous double ſt. ver. 240.
33	—	5 (r) Herculis	f	3	30	n	1	30	1	eF. pL. partly ver. 240.
34	15	5 (ξ) Leonis	f	13	0	ſ	0	10	1	eF. vS. completely ver. 240.
35 36	—	78 (ι) Leonis	f	20	30	ſ	0	15	1	Two. par. 3 or 4 diſt. Both eF. vS.
37	—	12 (l) Virgin	p	8	15	n	0	40	1	eF. vS. with 240. cL.
38	—	— —	f	11	15	n	0	20	1	vF. vS.
39	—	— —	f	12	45	n	0	15	1	vF. near ſome Bſt.
40	—	30 (ρ) Virg	p	11	15	n	0	31	1	eF. pL. eaſily overlooked.
41	—	— —	p	10	45	n	1	14	1	vF.
42	—	— —	p	10	15	n	1	23	1	vF.
43	—	34 Virginis	p	6	6	ſ	0	4	2	vF. pL. lE. contains two ſt.
44	—	— —	p	3	30	ſ	0	25	2	The p. of 2. vF. S. Note.
45 46	—	71 Virginis	f	0	37	n	0	12	1	Two. miſtaken for one; but 240 ſhewed them both. cL. vF.
47	—	32 Bootis	p	25	0	ſ	0	47	1	vF. r. 2 or 3 ſt in it.
48	—	— —	f	3	30	n	0	27	1	eF.
49	19	62 (1 ο) Can.	p	14	33	ſ	1	5	2	F. cS. lE. np ſf. like 2 joined.

III.	1784	Stars.		M.	S.		D.M.		Ob.	Deſcription.
50	Mar. 19	45 (1A) Can	f	3	15	ſ	0	4	1	eF. ver 240. and cL. R.
51	—	27 (ν) Leonis	p	7	0	n	0	21	1	Two. np. ſf. 6 or 7′ diſt. Both eF. p is the largeſt.
52										
53	—	34 Leonis	f	1	0	ſ	0	41	2	eF. S. lE. r. 3 or 4 ſt in it.
54	—	52 (K) Leon	p	10	45	ſ	1	27	1	eF. cL. R. r. no N.
55	—	46 (*i*) Leoni	f	4	18	n	0	3	2	vF vS. iR. r. ſome ſt. in it.
56	—	15 Bootis	p	13	0	ſ	0	40	1	eF. vS. E. r.
57	—	– –	p	10	30	ſ	0	28	1	eF. S. ver 240.
58	—	– –	p	8	30	ſ	0	43	1	eF. S. ver 240 and lE.
59	—	– –	p	6	15	ſ	1	10	1	eF. S. ver 240.
60	21	47 (δ) Cancri	f	20	0	n	0	23	1	vF. S. with 240 near Sſt.
61	—	– –	f	26	30	ſ	0	18	1	eF. 240 ſhewed 5 Sſt with nebuloſ.
62	—	– –	f	31	30	n	0	50	1	Two. nearly mer. Both vF. pS. R. lbM. r. with 240 cL.
63										
64	—	– –	f	36	0	n	0	52	1	eF. 240 ſhewed ſome Sſt with neb.
65	—	51 (*m*) Leon	p	38	15	ſ	0	33	1	vS. E. r. better with 240
66	—	– –	p	9	15	ſ	0	44	1	vF. S. E. r. the ſame with 240
67	—	– –	f	11	45	ſ	1	45	1	vF. nebul. betw. 2 ſt. 2′l. ver. 240
68	—	3 Comæ	p	1	45	ſ	0	40	1	2 vSſt with ſuſp. neb. 240 doubtf.
69	—	25 Comæ	p	5	0	ſ	0	18	1	vF. S.
70	—	27 Comæ	f	6	0	ſ	0	42	1	vF. not S.
71	—	42 Comæ	f	19	30	ſ	0	41	1	3 Sſt with ſuſpect. nebul. 240 left ſome doubt.
72	—	4 (τ) Bootis	p	10	15	ſ	1	26	1	eF. vS. ver 240 and cL.
73	—	5 (*r*) Herc	p	4	0	ſ	1	50	1	eF. vS. eaſily ver. 240.
74	—	48 Serpentis	p	1	15	n	0	5	1	vF. S. ver. 240
75	Apr. 8	70 (θ) Leonis	p	12	24	ſ	1	7	1	eF. not S.
76	—	– –	f	4	0	ſ	0	41	1	eF. pL. eaſily ver. 240.
77	—	94 (β) Leonis	f	12	12	ſ	1	12	1	eF. pL. R. r.
78	—	6 Comæ	f	17	18	ſ	0	19	1	vF. r. by moon-light.
79	12	73 (*n*) Leonis	p	5	6	ſ	1	25	1	eF. not L. lE. r.
80	—	– –	f	18	36	ſ	0	48	1	vF. vS. R. bM. ſtellar. ver. 240
81	—	– –	f	22	35	ſ	1	11	2	vF. vS. R. ſtellar.
82	—	41 Virginis	p	1	42	n	1	7	1	vF. S. E. r.
83	—	– –	f	6	18	n	0	0	1	vF. S. iF. r.
84	—	70 Virginis	p	3	42	ſ	0	4	1	eF. vS. ſtellar. ver. 240.
85	—	– –	f	6	12	n	0	1	1	Three. The two p. vF. S. R. The laſt vF. pL. R. Place of the 2d not taken.
86			..	..	..	..	..	..		
87			f	6	48	ſ	0	9		
88	13	56 Leonis	p	5	42	ſ	0	23	1	eF. no time to ver.
89	—	63 (χ) Leon	f	6	24	ſ	1	29	1	eF. a little doubtful.
90	—	3 (ν) Virginis	f	4	54	n	0	1	1	vF. vS. vlbM.
91	—	11 (ς) Virg	f	7	48	n	1	19	1	The f. of 2. eF. II. 17.

III.	1784	Stars.		M.	S.		D.M.		Ob.	Deſcription.
92 93	April 13	9 (ο) Virginis	f	16	15	ſ	2	4	2	Two. One vF. vS. The other juſt by. eF. eS. left doubtful.
94 95 96	—	– –	f	18	22	ſ	1	46	2	Three. All. eF, vS. R. In the 2d obſervation two of them were overlooked.
97	—	31 (1 *d*) Virg	p	17	9	n	0	42	2	The ſmalleſt of 2. eF. II. 144.
98	—	– –	p	3	6	n	0	0	1	eF. eS. The place not accurate.
99	—	32 (2 *d*) Virg	f	47	36	ſ	0	33	1	eF. S.
100	—	– –	f	50	42	ſ	1	8	1	eF. E.
101	—	– –	f	51	0	ſ	0	23	1	eF. pL. R. er. The ſt almoſt viſible
102	15	2 (1ξ) Virgin	p	1	48	n	1	54	1	eF. pL.
103	—	– –	f	0	24	n	0	58	1	vF. r.
104	—	4 (2ξ) Virgin	p	2	12	n	0	19	1	vF. vS. left doubtful. Twilight.
105	—	31 (1 *d*) Virg	p	1	52	n	1	35	2	eF. vL. lbM.
106	—	33 Virginis	f	7	30	n	0	8	1	vF. pL. vlbM. r.
107	17	48 Leonis	f	6	54	ſ	0	8	1	eF. pL. a little doubtful. Twil.
108	—	63 (χ) Leonis	p	13	18	n	1	7	1	eF. eS. r.
109	—	90 Leonis	f	5	18	n	0	49	1	8 or 10′ ſp. II. 161. vS. ſtellar. not ver.
110	—	20 Bootis	f	1	54	ſ	2	29	1	vF. vS. lE. ver. 240.
111	18	58 (*d*) Leon	f	8	36	..	..	..	1	vF. vS. r. ver. 240.
		84 (τ) Leon	..	..	..	n	1	43		
112	24	74 (φ) Leonis	f	10	6	ſ	1	52	1	eF. cL. R. r. near vBſt. ☽ light.
113	—	– –	f	34	18	ſ	1	3	1	eF. eS. with 240. 2 vSſt and nebu.
114	25	28 Virginis	p	14	18	n	1	35	1	2 vSſt with nebuloſity with 240 left doubtful.
115	May 9	67 (α) Virg	f	1	12	ſ	1	10	1	vS. vF. ſtellar. ver. 240.
116	—	31 (ι) Libræ	p	8	48	n	0	15	1	vF. cL. nearly R. lm.
117 118	11	100 (λ) Virg	p	59	30	n	0	48	1	The two moſt f. of 3. That M. vF. vS. The moſt f. eF. eS. ver. 240. II. 193.
119	—	– –	p	55	42	n	0	29	1	eF. vS. ſtellar. ver. 240.
120	—	– –	f	6	24	n	0	9	1	eF. pL. iR. lb towards f. ſide.
121 122	14	9 (α) Libræ	p	27	0	ſ	0	36	1	Two np ſf. The f. eF. 1′ d. nearly R. The p. vF. vS. R. diſt. 5′.
123	15	18 Herculis	f	40	30	ſ	0	47	1	vF. pL. R. lbM.
124	—	– –	f	43	30	ſ	0	47	1	vF. ſtellar. ver. 240.
125	16	25 (ρ) Bootis	p	33	12	ſ	1	10	1	vF. S. iR. lbM. almoſt ſtellar.
126	—	– –	p	2	18	ſ	0	24	1	2 Sſt. with ſuſpected nebul. almoſt ver. 240.
127 128	—	28 (σ) Bootis	f	3	48	n	0	45	1	Two. 3′ diſt. par. The f. vF. vS. iR. The p. eF. vS. ver. 240.

III.

III.	1784	Stars.		M.	S.		D.M.		Ob.	Deſcription.
129	May 16	28 (σ) Bootis	f	17	48	n	0	3	1	Two. about 6′ diſt. Both eF. vS. R. ver. 240.
130										
131	—	— —	f	22	54	n	0	11	1	vF. E. cloſe to a ſt. contains 2 ſt.
132	17	36 (ε) Bootis	p	16	54	n	0	26	1	eF. S. lE. the ſame with 240.
133	—	— —	p	2	36	n	1	35	1	eF. cL. iR. lbM.
134	19	12 (d) Bootis	f	4	28	n	0	12	2	vF. pL. E. par. r.
135	—	— —	f	12	30	n	1	5	1	eF. vS. ſtellar. ver. 240.
136	—	— —	f	14	8	ſ	0	30	2	vF. S. E. nearly par. with 240 like two ſtel.
137	—	76 (λ) Hercu	p	2	54	n	0	22	1	vF. not S. iE.
138	2:	20 (1γ) Libr	f	13	36	n	1	9	3	Two. nearly par. 7′ diſt. Both vF. not vS. R.
139										
140	June 11	27 (β) Hercu	p	23	30	ſ	0	51	1	vF. vS. r. ver. 240. np. pBſt.
141	July 12	16 (ψ) Capri	p	20	42	n	0	33	1	vF. cL. lE. lbM. 240. ſame.
142	21	70 Aquilæ	p	3	39	n	0	31	2	vF. E. about 2′ l.
143	Aug. 7	35 (2ν) Sagit	p	0	0	ſ	0	3	1	3 vSſt with ſuſpected nebuloſity.
144	Sept. 5	39 (b) Cygni	p	21	18	n	1	20	1	Some eSſt. with neb. iE. ver. 240.
145	10	10 (κ) Pegaſi	p	25	48	n	0	53	2	vF. lE. ſtellar.
146	—	69 Pegaſi	f	11	24	n	1	53	1	vF. E. ſome Sſt. with nebuloſity.
147	—	85 Pegaſi	f	7	54	ſ	1	13	1	2 or 3 ſt. with ſeeming nebuloſity.
148	11	28 Androm	p	4	12	ſ	0	32	1	vF. pL. lbM.
149	—	31 (δ) Andr	f	4	24	ſ	0	15	1	eF. vS. R.
150	—	2 (α) Trang	p	18	48	n	1	4	2	Near V. 18. vF. SR. bM.
151	—	6 (ι) Trang	p	7	0	ſ	1	18	1	vF. vS. ſtellar. betw. vL. and Sſt.
152	—	39 Arietis	p	8	12	n	0	49	1	vF. pS. of equal light.
153	1:	40 Andr	p	13	0	n	0	29	1	vF. pL. lE. vlb. towards f ſide.
154	—	— —	f	9	18	n	0	20	1	Two. Both eF. vS. The f is the largeſt.
155										
156	—	43 (β) Andr	f	13	6	ſ	2	8	2	Three forming a rect. triangle. In the legs eF. vS. at the rectangle vF. pL.
157										
158										
159	—	40 Androm	f	20	6	n	1	30	1	Two. Both eF. S. but unequal.
160										
161	—	17 (τ) Perſei	p	14	30	ſ	1	47	2	vF. S. iE. r.
162	—	21 Perſei	p	13	42	n	0	30	1	Two. Both vF. pS. R. lbM.
163				13	18		0	32		
164	—	— —	f	15	36	ſ	1	19	1	eF. vS. 240 left a doubt.
165	13	66 (ν) Cygni	f	43	0	n	0	4	1	5 or 6 ſt. forming a parallelogr. with mixed nebul. ver. 240.
166	—	— —	f	78	18	ſ	0	47	1	eF. vS. E. nf. & 4 or 5′ diſt. from I. 53.
167	—	43 (β) Andr	f	15	30	ſ	2	12	1	Two. Both ſtellar.
168										
169	—	— —	f	15	12	ſ	1	46	1	ſtellar.
170	—	— —	f	16	30	ſ	1	31	1	ſtellar.

III.	1784	Stars.		M.	S.		D.	M.	Ob.	Deſcription.
171	Sept. 13	43 (β) Andr	f	17	30	ſ	0	56	1	ſtellar.
172 173	—	— —	f	18	0	ſ	2	8	1	Two. Both vS. ſtellar. a little doubtful.
174	—	3 (ι) Triang	p	25	24	n	0	22	1	ſtellar. ver. 240.
175	—	— —	p	12	48	n	2	29	1	ſtellar.
176	—	— —	p	6	6	n	1	0	1	eF. ſtellar. 240 left ſome doubt.
177	—	9 (γ) Triang	f	9	36	ſ	0	17	1	vF. cL. iR. r. 2 or 3′ d.
178	—	17 (γ) Perſei	f	9	6	n	0	13	1	vF. pL. R. SB place M.
179	15	6 (β) Arietis	p	3	0	n	1	30	1	vF. pL. lE.
180	18	40 Pegaſi	p	3	0	n	0	47	1	eF. vS. R. n. cLſt.
181	—	65 Pegaſi	p	6	48	ſ	1	49	1	vF. vS. R. ver. 240.
182	—	40 Pegaſi	f	38	24	ſ	0	51	2	4 or 5 Sſt. with nebul. 240 doubt.
183	—	89 (χ) Pegaſi	f	0	30	ſ	1	38	1	eF. S. iE.
184	20	11 Piſcium	p	17	44	ſ	0	22	2	eF. vS. ſtellar. ver. 240.
185	—	— —	p	12	50	ſ	0	32	2	vF. E. er. 3 Fſt. viſible in it.
186	—	20 Piſcium	p	29	15	ſ	1	41	1	eF. vS.
187	—	— —	p	14	39	n	0	1	1	eF. ſtellar. ver. 240 and cL.
188	—	— —	p	13	33	ſ	0	9	1	eF. ſtellar. juſt like 187.
189	—	— —	p	8	15	ſ	1	52	1	eF.
190	—	29 Piſcium	f	4	54	ſ	0	40	1	vF. vS.
191	—	34 Ceti	p	9	12	ſ	1	53	2	vF. mE.
192	—	72 Ceti	p	17	24	ſ	1	43	1	eF. S. ver. 240. with difficulty.
193	—	— —	p	12	12	ſ	2	6	1	eF. ver. 240. with difficulty.
194	—	81 Ceti	f	38	6	n	0	55	1	eF. eS.
195	—	— —	f	42	42	n	0	49	1	eF. eS. ver. 240.
196 197	—	— —	f	47	0	n	0	36	1	Two Both eF. ver. 240 but juſt ſuſpected with 157.
198	Oct. 6	12 (q) Perſei	p	3	3	n	0	40	2	cB. mE. vgmbM. near 4′ l.
199	7	27 (κ) Perſei	p	8	27	n	0	2	2	The f of 2. vF. iF. pS. II. 239.
200	14	53 Piſcium	f	4	24	n	1	13	2	2 Sſt with nebuloſity ver. 240.
201	—	19 Arietis	f	4	6	ſ	0	47	1	vF. vS. E. ſ. pcſt.
202	15	47 Piſcium	p	83	54	ſ	1	15	1	eF. vS. ſtellar. ver. 240.
203	—	— —	p	78	18	n	0	18	1	vF. cL. E. 2′ l.
204	—	59 Piſcium	f	0	42	n	0	2	1	vF. S. ſp. 2 vSſt.
205	—	92 Piſcium	p	5	30	ſ	0	10	1	eF. ver. 240. diſcovered in gaging.
206	—	— —	p	3	30	ſ	1	20	1	eF. S.
207	—	8 (ι) Arietis	f	5	12	n	0	32	1	eF. vS. ſtellar. plainly. ver. 240.
208	—	— —	f	6	30	ſ	1	49	1	eF. vS. iR. juſt f. pBſt.
209	16	17 Delphini	f	18	6	ſ	0	11	1	vF. S. R.
210 211	—	54 (α) Pegaſi	p	2	48	n	0	46	1	Two. The p. vF. S. lE. The f. vF. vS. ſtellar.
212	—	— —	f	21	6	ſ	0	59	1	eF. eS. ver. 240. completely though with difficulty.
213	—	— —	f	27	36	n	0	40	1	eF. eL. ver. 240. betw. 2 Bſt.

III.	1784	Stars.		M.	S.		D.M.		Ob	Deſcription.
214	Oct. 16	31 Arietis	p	36	48	n	1	24	1	vF. ſtellar. ver. 240.
215	—	— —	p	36	6	n	0	6	1	eF. ſtellar. diſcovered by 240.
216	18	46 (ξ) Pegaſi	f	3	15	ſ	0	37	3	Two. The p. vF. pS. R. vlbM.
217				3	25		0	32		The f. vF. pS. R. vlbM.
218	—	58 (*n*) Pegaſi	f	13	51	n	0	4	1	eF. pS. lE.
219	19	15 Delphini	p	5	24	n	0	2	1	eF. vS. ſtellar ver. 240. with dif.
220	—	66 Pegaſi	p	10	10	n	0	23	4	F. R. bM. 1′¼ d.
221	—	— —	p	7	10	n	1	0	2	vF. S.
222	—	— —	p	7	7	n	0	54	2	vF. S. R.
223	20	7 (*b*) Ceti	f	23	12	ſ	1	1	1	vF. lE. or oval. 1′ d. np. 2 pBſt.
224	—	1 (1 τ) Erid	p	21	42	ſ	2	11	2	vF. S. iR.
225	–	15 (δ) Lepor	f	6	24	n	0	49	1	eF. E. r. near 1′ l. ver. 240.
226	21	70 (*q*) Pegaſi	p	1	50	ſ	0	18	2	vF. vS. ſtellar ver. 240.
227	Nov. 7	64 Ceti	p	2	24	ſ	0	37	1	2 or 3 Sſt. with neb. nearly ver. 240
228	—	73 (2ξ) Ceti	f	12	54	n	0	17	1	Two about 1′ diſt. The p. eF. vS. ver. 240. The f. eF. eS. 240. doubtf.
229										
230	12	55 (*l*) Pegaſi	p	3	36	ſ	0	29	1	eF. eS. 240 left ſome doubt.
231	—	31 (1 *c*) Piſc	p	9	0	ſ	1	0	1	Two. Both vF. ſtellar.
232										
233	—	— —	p	8	27	ſ	1	0	2	eF. pL. glbM.
234	16	43 (γ) Canc	p	11	24	n	1	6	1	vF. ſtellar.
235	—	— —	p	3	20	n	2	4	1	eF. S. ver. 240.
236	—	4 (λ) Leonis	p	23	22	ſ	1	37	1	eF. lE. betw. 2 pBſt. ver. 240.
237	17	33 Pegaſi	f	12	54	n	0	46	1	eF. vS.
238	—	66 Pegaſi	p	6	6	n	1	10	1	eF. eS. ver. 240. with difficulty.
239	—	4 Eridani	p	32	26	ſ	1	1	1	vF. S. 1′ dia. or more.
240	20	12 Leporis	p	7	55	ſ	0	59	1	vF. vS. ſtellar.
241	—	— —	f	3	39	n	0	23	1	eF. vS. lE. par.
242	—	15 (ι) Nav	f	68	16	n	0	53	1	vF. lE. S. 1′ d.
243	Dec. 2	56 Pegaſi	p	9	16	n	0	42	1	vF. S. er.
244	9	48 Ceti	P	48	34	n	0	27	1	eF. vS. E.
245	—	15 Eridani	p	15	49	ſ	0	27	1	vF. cL. iE. r. unequally B.
246	—	19 Eridani	p	1	38	n	0	50	2	vF. E. equally B.
247	—	— —	f	6	5	ſ	1	4	1	eF. vS.
248	—	27 Eridani	p	4	23	n	1	7	1	vF. vS. lE.
249	—	— —	p	2	19	n	1	18	1	vF. vS.
250	13	89 (*f*) Piſci	f	2	25	ſ	0	14	1	Two. nearly par. 4 or 5′ diſt.
251				..	..		..	..		Both vF. vS. R.
252	—	— —	f	3	42	n	1	38	1	vF. pL. iR. lbM.
253	—	— —	f	6	48	n	0	11	1	eF. cL. E.
254	—	15 Sextantis	p	14	34	n	1	52	2	vF. E. np ſf. 5′ l. ¼′ b.
255	—	7 Sextantis	f	20	27	n	0	42	1	vF. vS. p. triangle of Bſt.
256	20	13 (ζ) Can. mi	f	26	5	ſ	0	48	1	vF. vS. ver. 240.

III.	1784	Stars.		M.	S		D.M.		Ob.	Deſcription.
257	Dec. 20	13 (ζ) Can. mi	f	44	59	ſ	0	55	1	eF. pL. iF.
258	—	10 (*r*) Virgin	p	5	2	ſ	0	7	2	vF. S. E.
	1785									
259	Jan. 6	70 Ceti	p	10	34	ſ	0	38	1	eF. eS. iF.
260	—	– –	p	7	10	n	0	4	1	eF. vS. ſtellar.
261	—	75 Ceti	p	3	46	ſ	0	6	1	vF. cL.
262	—	94 Ceti	p	1	16	ſ	1	15	1	eF. ver. 240 with difficulty.
263	—	24 Eridani	p	3	22	ſ	0	11	1	eF. ſtellar. or lE. almoſt ver. 240.
264	—	28 (A) Hydr	p	26	48	n	1	19	2	vF. vS. R. ver. 240.
265	10	45 (θ) Ceti	f	32	28	ſ	0	46	1	eF. ſtellar. ver. 240.
266	—	– –	f	31	6	ſ	0	43	1	vF. lE. ver. 240.
267	Feb. 4	14 (ζ) Lepor	f	0	1	ſ	1	56	1	vF. pS. iE. bM.
268	6	11 (α) Lepor	p	27	51	ſ	0	31	1	eF. vS. ſtellar. ver. 240. eaſily.
269	—	19 Leporis	p	32	23	n	1	11	1	eF. vS. ſtellar. ver. 240. eaſily.
270	—	– –	p	20	0	n	1	28	1	vF. eS. ſtellar. ver 240 difficulty.
271	—	8 (3 ν) Canᶜ	f	8	0	n	0	4	1	3 or 4 Sſt with neb. vF. ver. 240.
272	7	6 (3 *b*) Crater	p	58	39	n	1	21	1	vF. pS. iF. vlbM.
273	—	– –	p	55	43	n	0	39	1	eF. vS. iF.
274	—	31 Crateris	p	4	40	ſ	0	14	1	vF. pL. iF.
275	8	12 Hydræ	f	20	30	ſ	1	49	1	vF. vS. bM. ½' ſ. Sſt.
276	—	38 (κ) Hyd	p	9	20	ſ	0	26	1	vF. vS. ſtellar. 240. the ſame.
277 278	—	39 (1 *υ*) Hyd	p	5	0	n	0	30	1	Two. 3 or 4' diſt. The moſt n. vF. S. The ſ. vF. vS. Both ſtell.
279	—	8 (η) Corvi	p	31	26	n	0	16	1	eF. pL. better with 157 than 240.
280	—	– –	f	18	44	n	1	51	1	½' p. II. 298. eF. eS. ſtell. 240. doubtful.
281	—	– –	f	20	38	n	0	46	1	vF. pS. r.
282	—	53 Virginis	f	7	12	n	1	12	1	vF. mE. ſf np. v narrow.
283	17	41 (ω) Bootis	p	27	54	n	0	27	1	vF. vS.
284	Mar. 5	25 (*f*) Virg	p	54	12	ſ	0	19	1	vF. S. iE. lbM.
285	—	88 Virginis	f	8	45	n	1	17	1	eF. vS.
286	—	99 (ι) Virg	p	9	22	n	0	31	1	vF. L. b towards n.
287	—	– –	p	7	58	ſ	0	7	1	vF. pS. iF.
288	6	15 (ι) Navis	f	11	16	ſ	1	7	1	vF. cL. er. ſome of the ſt. viſ.
289	10	6 (3 *b*) Crat	p	69	14	ſ	0	25	2	F. vS. large ſtellar. lbM.
290	—	2 (ε) Corvi	p	16	1	n	2	3	1	eF. pL. broadly E. nearly par.
291	11	75 Cancri	p	2	53	ſ	1	13	1	vF. pL. R. bM.
292	12	46 Cancri	p	11	46	ſ	1	14	2	vF. pL. R. lbM. r.
293	—	23 Leonis	p	17	46	ſ	0	22	1	eF. eS. ver. 240.
294	13	57 (2 ι) Canc	p	2	44	n	0	15	1	vF. vS. R. bM. large ſtellar.
295	—	72 (τ) Canc	f	5	47	n	0	24	1	vF. vS. R. nf. 2pBſt.
296	—	– –	f	8	42	n	1	17	1	vF. S. R. lbM.
297	—	15 (*f*) Leon	p	13	8	ſ	0	34	1	eF. eS. 240 left a doubt.
298	—	18 Leonis min	p	20	56	ſ	0	44	2	vF. vS. iR. lbM.

III.	1785	Stars.		M.	S.		D.M.	Ob.	Deſcription.
299	Mar. 13	13 Can. ven.	p	40	51	ſ	0 27	1	eF.
300	—	– –	p	40	19	ſ	1 28	1	The moſt f of 3. vF. II. 322. 323.
301	—	– –	p	28	58	ſ	1 41	1	vF. vS R.
302	—	– –	p	27	40	ſ	1 2	1	eF. vS.
303	—	– –	f	5	43	ſ	1 43	1	eF. vS. ver. 240.
304	—	– –	f	6	26	ſ	1 47	1	eF. vS. ver. 240.
305	—	– –	f	11	0	ſ	1 9	1	vF. vS. lE.
306 307	—	– –	f	16	12	n	0 6	1	Two. The p. vF. vS. The f. 7 or 8′ nf the firſt. vF. vS.
308	—	– –	f	17	29	n	0 13	1	vF. S.
309	—	– –	f	18	31	n	0 34	1	eF. vS.
310	—	49 (δ) Bootis	p	43	12	ſ	1 32	1	vF. vS. iF.
311	16	11 Urſæ min	p	24	18	n	1 18	1	vF. S. iR. between 2 pSſt.
312	—	– –	p	19	9	n	2 6	1	eF. vS. lE. 2 vSſt in it.
313	—	13 (γ) Ur. mi	f	27	8	n	0 12	1	vF. vS. lE.
314	—	– –	f	49	18	n	0 24	1	eF. vS. lE. er.
315	Apr. 3	27 Urſæ	f	3	42	n	0 46	1	eF. vS. ver. 240.
316	—	– –	f	51	42	n	1 43	1	eF. pS mE. r.
317	—	– –	f	65	18	n	1 19	1	vF. vS.
318	—	– –	f	69	10	n	0 20	1	vF. pL. r.
319	—	7 (β) Urſ. mi	p	32	2	n	2 26	1	eF. not verified.
320	6	72 Leonis	f	26	8	n	1 44	2	vF. vS. ſtellar.
321	—	4 Comæ	p	22	54	n	0 15	1	vF. pS.
322	—	– –	p	19	8	n	0 18	1	vF ſtellar.
323 324	—	– –	p	14 . . .	43 .	ſ	0 40	1	Two. The ſp. vF. lE. The nf. eF. 5 or 6′ diſt.
325	—	– –	p	13	46	ſ	0 45	1	eF. vS.
326	—	– –	p	5	47	ſ	0 17	1	eF. vS. ver. 240. diſcovered in gaging.
327	—	– –	p	1	45	n	0 33	1	vF. pS.
328	—	31 Comæ	p	6	28	ſ	0 25	2	F. S.
329	—	21 (g) Comæ	f	14	45	n	2 26	2	vF. S.
330	10	36 (ζ) Leonis	f	4	54	n	0 29	1	vF. pS. vlbM. iR.
331	—	41 Leonis mi	p	12	16	n	1 36	1	vF. vS. vlbM.
332	—	54 Leonis	f	2	46	ſ	0 34	1	vF. 1′ n. Sſt.
333	—	72 Leonis	f	2	48	n	0 18	1	vF. vS. ver. 240.
334	—	– –	f	3	17	n	0 23	1	vF. S.
335 336	—	– –	f	7	12	n	1 12	1	Two. 2 or 3′ diſtant. Both vF. vS. the moſt f. fainteſt.
337	—	– –	f	9	34	n	0 52	1	vF. S.
338	—	– –	f	25	56	ſ	0 38	1	vF. vS. 240. the ſame.
339	—	– –	f	26	30	n	1 44	1	vF. vS. 240. the ſame.
340	—	– –	f	28	36	ſ	0 19	1	vF. vS. pL. two ſtellar. ſuſpected near it.

III.	1785	Stars.		M. S.		D.M.	Ob.	Deſcription.
341	Apr. 10	7 (*b*) Comæ	p	26 41	n	0 56	1	vF. vS. ver. 240. eaſily.
342	—	– –	p	22 55	ſ	0 32	1	vF. vS. lE.
343	—	– –	p	20 7	ſ	0 4	1	vF. vS. 240. the ſame.
344 345	—	– –	p	18 31	ſ	0 43	1	Two. 5 or 6′ diſtant. Both eF. vS. ver 240.
346	—	40 Comæ	f	1 38	n	2 8	1	eF. pL. lE. ver. 240.
347	—	12 (*d*) Bootis	f	7 40	ſ	1 17	1	vF. lE. S.
348	11	23 Leonis min	f	3 12	ſ	1 38	1	eF. lE. a little doubtful.
349	—	39 Leonis min	p	9 28	n	1 18	1	eF. 240 ſhewed a few Sſt. with neb. but doubtf.
350	—	44 Leonis min	f	17 36	n	0 35	1	vF. S.
351 352	—	– –	f	20 58	n	0 51	1	Two. Both vF. vS. the moſt ſ. is the fainteſt.
353	—	– –	f	53 4	n	0 26	1	eF. 240 left it doubtful.
354	—	14 (*b*) Comæ	p	28 29	n	0 43	1	vF. vS. diſcovered in gaging.
355	—	– –	p	21 41	ſ	0 16	1	vF. S. pmE.
356 357	—	– –	p	17 40	n	1 55	1	Two of 3. the place is that of II. 371. Both vF. mE. A 4th ſuſpected.
358 359 360	—	– –	p	14 24	n	1 55	1	Three of a quartile. The place is that of II. 372. All vF. vS. and all within 3′.
361	—	– –	p	0 40	n	0 18	1	vF. vL.
362	—	15 (*c*) Comæ	f	3 2	ſ	1 3	1	eF. cL. 4 or 5′ l. 2′ b.
363	—	41 Comæ	p	6 16	n	0 23	1	vF.
364	—	– –	p	5 24	n	0 25	1	vF.
365	—	– –	f	1 8	n	0 41	1	vF.
366	—	– –	f	2 26	n	1 18	1	vF. pS.
367	—	43 Comæ	f	1 24	ſ	0 2	1	vF. pL.
368	—	– –	f	11 2	ſ	0 53	1	vF. mE. $1\frac{1}{2}'$ l. r. diſcov. gaging.
369	—	– –		25 41	ſ	0 29	1	eF. vS. 240 left a little doubt.
370	—	– –	f	28 8	n	0 31	1	vF. S. mE. nearly mer.
371	—	14 (ι) Coron	p	13 52	ſ	1 8	1	vF. S. R. ver. 240 eaſily.
372	13	93 Leonis	p	1 25	n	0 25	1	vF. cL. moon-light.
373	14	11 Libræ	f	1 18	ſ	0 12	1	vF. juſt n. Sſt.
374	—	11 Serpentis	p	12 8	ſ	1 18	1	eF. pL. r.
375	25	93 Leonis	p	7 28	n	0 7	2	vF. vS. r.
376	26	– –	p	5 57	n	0 5	2	eF. vS.

Fourth claſs. Planetary nebulæ.

Stars with burs, with milky chevelure, with ſhort rays, remarkable ſhapes, &c.

IV.	1782	Stars.		M.	S.		D.	M.	Ob.	Deſcription.
1	Sept. 7	13 (ν) Aquarii	p	5	24	n	0	2	11	vB. nearly R. planetary not well defined diſk.
	1783									
2	Dec. 26	13 Monocer	f	6	4	n	1	27	4	cB. fan-ſhaped. about 2′ l. from the center. Fig. 7.
	1784									
3	Jan. 16	15 Monocer	p	8	18	n	0	15	4	pB. m. like a ſt. with an electrical bruſh. Fig 8.
4	Feb. 22	69 Leonis	f	10	3	ſ	1	3	2	eF. S. like an ſt. with a vF. bruſh ſp. 240 ſhews the ſt.
5	—	29 (γ) Virg	p	9	0	n	1	33	2	A pBſt. with a m. ray ſ. par. 15 or 20′ l. Fig. 6.
6	23	59 (c) Leonis	p	9	0	ſ	0	18	1	F. L. C. A central B. point with eF. m. chev.
7	Mar. 14	51 (m) Leon	f	17	0	ſ	0	39	2	F. pL. m. between 2 Bſt. like an electrical bruſh to the moſt n. but is not connected. R.
8 } 9	15	34 Virginis	p	10	12	ſ	0	51	2	A double Nebula. The che. run into each other. cloſe. not vF.
10	21	51 (m) Leo	p	21	15	ſ	1	48	1	A pcſt. with a vF. bruſh nf. with 240 2 vSſt. viſible in it, but not connected.
11	May 21	51 (e) Ophiu	p	1	42	n	0	14	2	pB. R. p. well defined planetary diſk. 30 or 40″ d.
12	24	3 (p) Sagitt'	f	22	0	n	1	47	1	F. L. iR. inclining to m. 3 or 4′ d. like a bruſh to a np. ſt. but probably unconnected.
13	July 17	39 (b) Cygni 21 Vulpecu	p f	8 2	6 6	ſ n	1 1	35 51	2	pF. exactly R. of equal light. the edges p. well def. 1′ d. See note.
14	21	27 (d) Aquilæ	p	6	6	ſ	1	45	2	vF. of equal light. r. 1′ d. in the midſt of numberleſs ſt. of the milky way.
15	Sept. 8	21 (α) Andr	f	2	6	ſ	1	21	1	A Fſt. with S. chev. and 2 burs.
16	16	16 (η) Sagittæ	f	17	12	n	0	1	2	pB. perfectly R. pretty well defined. ¾′ d. r.
17	20	81 Ceti	f	36	30	n	0	36	1	A Sſt. with a vF. nebulous bruſh 1½ or 2′ l. diſcovered with 240.

IV.	1784	Stars.		M.	S.		D.M.		Ob.	Deſcription.
18	Oct. 6	14 Androm.	p	6	11	n	3	16	4	B. R. a planetary p. well defined diſk. 15″ dia^{r} with a 7 feet reflector.
19	16	5 Monoc.	p	7	6	ſ	0	10	1	A ſt. of the 9 magnitude, with m. chev. i elliptical.
20	—	– –	p	3	42	n	0	3	1	A ſt. of the 11 or 12 mag. affected like the foregoing, but vF.
21	Nov. 20	12 Leporis	p	8	48	n	0	24	1	vS. ſtellar. vBN. and vF. chev. not quite central.
22	Dec. 9	7 (ξ) Navis	f	3	10	ſ	1	28	2	L. pB. R. er. 6 or 7′ d. a faint red colour viſible. A ſt. 8 mag. not far from the center, but not connected. 2d ob. 9 or 10′ d.
	1785									
23	Jan. 6	75 Ceti	p	4	40	ſ	0	6	1	cB. a vBN. with a chev. of 3 or 4′ d.
24	—	50 (ζ) Orio	f	0	57	ſ	0	17	1	A Bſt. with m. chev. 5′ l. 4′ b.
25	31	19 Navis	p	67	0	n	1	15	1	A pcſt. with vF. and vS. m. chev. iF.
26	Feb. 1	34 (γ) Erid	f	16	16	n	0	49	2	vB. perfectly R. or vl. elliptical. planetary but ill defined diſk. 2d obſ. r. on the borders, and is probably a very compreſſed cluſter of ſtars at an immenſe diſtance.
27	7	6 (3*b*) Crater	p	28	39	n	1	25	2	Beautiful, brilliant, planetary diſk ill defined, but uniformly B. the light of the colour of Jupiter. 40″ d. 2d obſ. near 1′ d. by eſtimation.
28	—	31 Crateris	f	1	0	n	0	47	1	pB. L. opening with a branch, or two nebulæ very faintly joined. The ſ. is ſmalleſt.
29	8	4 (ν) Crateris	f	3	36	n	0	16	1	A Sſt. with an eF. bruſh p. perceived in gaging. ver. 240.

Fifth claſs. Very large nebulæ.

V.	1783	Stars.		M.	S.		D.M.		Ob.	Deſcription.
1	Oct. 30	18 (ε) Piſ. auſt.	f	128	17	n	1	39	6	cB. mE. ſp nf. mbM. Above 50′ l. and 7 or 8′ b. C. H. See note.
	1784									
2	Jan. 24	10 (*r*) Virgin	f	24	46	n	0	17	4	cB. mE. np ſf. mbM. er. 9 or 10′ l with a branch towards the np.

V.	1784	Stars.		M.	S.		D.M.		Ob.	Deſcription.
3	Jan. 24	75 Leonis	f	104	0	ſ	0	24	1	eF. vL. er. R. 7 or 8′ d.
4	Feb. 23	7 (*b*) Virgin	f	8	15	ſ	0	45	2	vF. R. 5 or 6′ d.
5	Mar. 14	11 Comæ	f	0	45	n	0	32	1	L. E. r. 6 or 7′ l.
6	21	4 (τ) Bootis	p	0	45	ſ	1	6	1	vL. eF. r.
7	Apr. 8	52 (K) Leonis	p	3	0	n	0	41	1	vL: F. r. almoſt R.
8	—	73 (*n*) Leonis	f	4	34	n	0	18	3	B. E. almoſt par. but l. np ſf. near 15′ l.
9	May 22	51 (*e*) Ophiu	f	32	48	ſ	0	40	1	L. E. broad. m. F.
10 11 12	July 12	5 (*i*) Sagitt	f	2	42	n	0	49	1	Three nebulæ, faintly joined, form a triangle. In the middle is a double ſt. vF. and of great extent.
13	—	— —	f	4	54	n	0	39	1	Extenſive m. neb. divided into 2 parts. the moſt n. above 15′. The moſt ſ. followed by ſtars.
14	Sept. 5	52 (*k*) Cygni	f	11	24	n	0	44	2	Branching nebuloſity, extending in R.A. near 1½ deg. and in P.D. 52′. The f. part divides into ſeveral ſtreams uniting again towards the ſ.
15	7	— —	f	0	0	n	0	0	3	Extended; paſſes thro' *k* Cygni. By the Newtonian view above 1 degree l. By the *Front-view* near 2 deg. l. See note.
16	11	28 Androm.	p	11	12	n	0	17	1	eF. 5 or 6′ d.
17	—	2 (α) Triang	p	18	48	n	0	55	2	m. nebuloſity. not leſs than ½ deg. broad. perhaps ¾ degree long, but not determined.
18	Oct. 5	35 (ν) Andr	p	9	11	n	0	37	4	vB. mE. 30′ l. 12. b. C. H.
19	6	26 (β) Perſei	p	45	11	n	1	16	3	cB. mE. above 15′ l. 3′ b. a black diviſion 3 or 4′ l. M.
20	20	7 (*b*) Ceti	f	33	9	ſ	1	48	1	A ſtreak of light, nearly mer. 26′ l. 3 or 4′ b. pB.
	1785									
21	Jan. 31	18 (μ) Canis	f	22	18	n	1	2	2	A broad E nebuloſity. forms a parallelogram with a ray ſouthwards; the parall. 8′ l. 6′ b. vF.
22	Feb. 7	61 Virginis	f	10	59	n	0	17	1	mE. ſf np. 5 or 6′ l. pF.
23	Apr. 3	27 Urſæ	f	13	18	n	0	0	1	L. F. lE. r. 6 or 7′ l. 5 or 6′ b.
24	6	21 (*g*) Comæ	f	5	20	n	1	25	1	A lucid ray 20′ l. or more. 3 or 4′ b. np ſf. vBM. a beautiful appearance.

Sixth claſs. Very compreſſed and rich cluſters of ſtars.

Additional abbreviations	Cl. Cluſter.	com. compreſſed.
	ſc. ſcattered.	co. coarſely.

VI.	1783	Stars.		M.	S.		D.	M.	Ob.	Deſcription.
1	Nov. 19	63 (*p*) Gemi	f	11	0	n	0	12	3	A beautiful Cl. of many L. and com. S. ſt. about 12′ d.
2	Dec. 30	18 (ν) Gemi	f	27	10	ſ	2	9	3	A v. com. Cl. of eSſt. iF. 5 or or 6′ d.
	1784									
3	Jan. 24	12 Monocer	f	11	30	ſ	0	18	1	A Cl. of v. com. and eS. ſt. E.
4	—	4 Sextantis	f	5	30	ſ	0	5	1	A Cl. of v. com. S. ſt.
5	Feb. 11	31 (2ξ) Gem	p	31	0	ſ	0	15	1	A Cl. of v. com. S. 7 or 8′ d.
6	Mar. 8	67 Gemin	p	18	0	ſ	1	57	1	A Cl. of ſt. of various ſizes pm. com. M. p. rich.
7	14	42 Comæ	f	8	30	n	0	8	1	An eF. Cl. of eS. ſt. with r. neb. 8 or 10′ d. ver. 240. beyond doubt.
8	Apr. 25	26 (χ) Virg	f	23	44	ſ	0	6	1	A v. com. Cl. of ſt. 8 or 9′ d. e rich. iR. or lE.
9	May 17	11 Bootis	f	4	18	n	1	7	1	A Cl. of eS. and com. ſt. 6 or 7′ d. many of the ſt. viſible, the reſt ſo S. as to appear nebulous.
10	22	21 (α) Scorp	p	1	48	n	0	24	1	A v. com. and cL. Cl. of the ſmalleſt ſtars imaginable. all of a duſky red colour. the next ſtep to an er. neb.
11	—	39 Ophiuchi	p	13	24	ſ	0	26	1	A fine miniature of the 19 nebula of the Connoiſſ. des Temps (which is a Cl. of v. com. ſt. much accumulated M. 4 or 5′ d. all the ſt. red.) 2 or 2′½ d. the ſt. F. red.
12	24	43 Ophiuchi	p	12	42	n	1	36	1	Another miniature Cl. like the preceding, but rather coarſer.
13	June 24	10 (γ)¹ Sagitt	p	14	48	n	0	18	1	A Cl. of S. and p. com. ſt. of ſeveral mag. 5 or 6′ d. not v. rich.
14	July 11	9 Vulpec	p	4	10	n	0	33	1	A Cl. of eS. and v. com. ſt. a parallelogram of 4′ l. 2′ b. mer.
15	July 12	34 (σ) Sagitt	p	6	54	n	0	27	1	A ſuſpected Cl. of vFſt. of conſiderable extent. not ver.
16	Aug. 18	12 (γ) Sagittæ	p	4	18	ſ	1	32	1	A vS. Cl. of com. ſt.
17	Nov. 16	42 (1ω) Gem	p	54	53	ſ	0	29	2	A v. rich Cl. of v. com. and eSſt. 4 or 5′ d. A miniature of the 35 Cl. of the Conn. des T. which it precedes 1′ 18″ and is 2′ n.
	1785									
18	Mar. 4	11 Monocer	f	27	15	ſ	0	2	4	A v. com. and rich Cl. of vSſt. iF. 8 or 9′ d.
19	10	24 (1ſt ι) Libr	f	5	0	ſ	1	16	1	A beautiful L. Cl. of the moſt minute and moſt com. ſt. of different ſizes. 6 or 7′ d. iR. F. red colour.

Seventh claſs. Pretty much compreſſed cluſters of large or ſmall ſtars.

VII.	1784	Stars.		M.	S.		D.M.		Ob.	Deſcription.
1	Jan. 18	90 (1 *c*) Tauri	f	11	0	ſ	1	30	2	A Cl. of L. ſcat. ſt. 10 or 12′ in extent, with a vacancy M.
2	24	8 Monocer	f	8	17	n	0	23	3	A beautiful Cl. of ſc. ſt. chiefly of 2 ſorts. the firſt L. the ſecond arranged in winding lines. contains the 12th Monoc.
3	Feb. 8	3 Leporis	p	72	30	ſ	0	30	1	A S. Cl. of com. ſt. ſome pL.
4	19	15 (2 *y*) Orio	f	3	6	n	1	10	2	A Cl. of pL. and p. com. ſt. c. rich. 20 or 25′ d. iR.
5	23	13 Monocer	p	3	15	ſ	0	28	1	A Cl. of com. ſt. of various mag. p. rich in Sſt. not R.
6	Mar. 16	50 Gemino	f	3	55	ſ	2	9	1	A p. rich and com. Cl. of ſt.
7	May 24	3 (*ρ*) Sagitt^{i}	f	15	54	ſ	0	8	1	A c. rich, but p. co. ſc. Cl. of ſt. l. more com. M.
8	July 17	41 (*i*) Cygni	f	5	42	ſ	2	1	5	A v. rich Cl. of pS. ſc. ſt. moſt of the ſame ſize. 20′ d.
9	19	12 Velpecu	p	0	5	n	0	30	2	A L. Cl. of p. com. ſt. moſt of one ſize.
10	Nov. 20	7 (*ξ*) Navis	f	5	56	n	0	40	3	A vL. Cl. of ſc. ſt. c. rich and com. more than 15′ d.
	1785									
11	Jan. 31	19 Navis	p	0	40	n	0	5	1	A c. rich Cl. of co. ſc. ſt. above 20′ d.
12	Feb. 4	6 Navis	p	31	59	n	1	25	4	A beautiful Cl. of p. com. ſt. near ½ deg. d. C. H.
13	6	2 (*β*) Canis	p	7	10	ſ	0	44	1	A Cl. of ſc. Sſt. not v. rich above 15′ d.
14	8	18 (*μ*) Canis	f	3	17	n	0	20	1	A Cl. of co. ſc. ſt. 20′ d.
15	Mar. 6	26 Canis	f	1	22	n	1	52	1	A S. Cl. of p. com. ſt. not v. rich.
16	—	– –	f	1	56	n	0	16	1	A Cl. of ſc. ſt. c. rich. 20′ d.
17	—	– –	f	6	26	n	1	1	2	A v. beautiful Cl. of pL. ſt. v. rich. contains the 30 Canis.

Eighth claſs. Coarſely ſcattered cluſters of ſtars.

VIII.	1783	Stars.		M.	S.		D.M.		Ob.	Deſcription.
1	Dec. 3	14 Navis	p	4	0	n	0	40	2	A Cl. of co. ſc. ſt. The place is that of the moſt com. part which is not M.
2	26	58 (*α*) Orion	p	8	28	n	1	16	2	A S. Cl. of vS. ſc. ſt.
3	—	13 Monocer	f	1	30	n	1	2	2	An E. Cl. of L. ſc. ſt.

VIII.

VIII	1784		Stars.		M.	S.		D.M.		Ob.	Deſcription.
4	Jan.	16	112 (*β*) Tauri	p	0	51	n	0	38	3	A Cl. of co. and i. ſc. pLſt.
5		18	15 Monocer	p	0	0	n	0	0	3	Double and attended by more than 30 cLſt.
6		24	8 Monocer	p	14	20	n	0	4	2	A Cl. of co. ſc. ſt. not rich.
7	Feb.	10	4 Orionis	p	4	0	ſ	1	7	1	A Cl. of L. and S. ſc. ſt. not rich.
8		15	97 (*i*) Tauri	p	5	28	n	0	13	2	A Cl. of cL. v. co. ſc. ſt. perhaps a projecting point of the m way.
9		19	24 (*γ*) Gemi	p	8	15	n	0	15	1	A Cl. of vm. ſc. ſt. of various magnit. near ½ deg. not rich.
10	Mar.	15	50 (2A) Canc	f	3	0	ſ	0	44	1	A Cl. of v. co. ſc. ſt. not rich.
11		16	50 Gemini	f	15	55	ſ	2	19	1	A Cl. of ſc. ſt.
12	June	16	1 (*m*) Aquilæ	f	1	42	n	0	2	1	A Cl. of v. co. ſc. ſt.
13		—	20 Aquilæ	p	12	48	ſ	0	56	1	A Cl. of co. ſc. ſt. not rich.
14		18	43 (*d*) Sagitt	p	44	48	n	1	54	1	A Cl. of ſc. pLſt.
15	July	15	63 Sagittarii	p	103	36	n	2	1	1	A Cl. of co. ſc. ſt.
16		17	12 (*φ*) Cygni	f	13	6	ſ	0	44	1	A Cl. of not v. com. ſt. cloſeſt M. It may be called (if the expreſſion be allowed) a forming Cl. or one that ſeems to be gathering
17		18	33 Vulpec	p	24	18	n	0	4	1	A Cl. of many L. ſc. ſt.
18	Sept.	4	61 (*φ*) Aquilæ	p	2	54	n	0	18	1	A S. forming Cl. of ſt.
19		—	– –	p	0	42	n	0	40	1	A Cl. of co. ſc. L. ſt. not rich.
20		9	18 Vulpec	f	1	0	ſ	0	27	1	A Cl. of co. ſc. ſt. not rich.
21		10	6 Vulpec	p	2	27	n	0	29	2	A Cl. of cL. co. ſc. ſt.
22		—	18 Vulpec	f	1	12	ſ	0	12	1	A Cl. of co. ſc. ſt.
23	Oct.	15	12 (*γ*) Delph	p	5	18	n	0	33	1	A Cl. of co. ſc. ſt.
24		—	67 (*ν*) Orion	f	1	0	ſ	0	46	1	A SCl. of pL. white ſt.
25		16	10 Monocer	f	0	0	ſ	0	0	1	The 10 Monoc. ſurrounded by by many Bſt.
26	Nov.	16	1 (H) Gem	p	2	16	n	0	3	1	A Cl. of ſt. of various magnit. not v. rich. 6 or 7′ d.
27		20	11 (*e*) Navis	p	36	41	n	0	46	1	A S. Cl. of ſc. ſt. not rich, nor v. com.
28	Dec.	5	54 (1λ) Orion	p	11	53	ſ	0	15	1	A Cl. of pL. ſc. ſt. not rich.
29		9	101 (4*b*) Aqu	f	32	30	n	0	11	1	A Cl. of a few co. ſc. L. ſt.
30		—	25 (*δ*) Canis	f	57	10	ſ	1	15	1	A vL. Cl. of many co. ſc. L. ſt.
	1785										
31	Jan.	6	19 Monocer	p	15	36	n	1	3	1	A L. Cl. of ſc. ſt. not v. rich.
32		10	26 Monocer	p	34	32	ſ	0	41	1	A Cl. of co. ſc. ſt. of many magn. p. rich. above 15′ d.
33		—	– –	p	32	50	ſ	1	15	1	A Cl. of ſc. L. ſt.
34		—	– –	p	26	36	ſ	0	52	1	An extenſive Cl. of ſc. ſt.
35		31	2 Navis	p	21	23	n	1	21	3	A Cl. of pL. ſc. ſt. p. rich. about 20′ l. crooked fig.

VIII.	1785	Stars.		M.	S.		D.	M.	Ob.	Deſcription.
36	Jan. 31	19 Navis	p	43	20	n	1	0	1	A forming Cl. of co. ſc. ſt. 20 or 30′ dia.
37	Feb. 4	6 Navis	p	16	47	n	1	43	2	A S. Cl. of p. com. ſt. of various ſizes. not v. rich.
38	—	2 Navis	p	8	55	n	0	10	2	A Cl. of p. com. L. and S. ſt. R. above 15′ d.
39	Mar. 4	11 Monocer	f	23	36	n	0	3	3	An extenſive Cl. of ſc. ſt. of various ſizes.
40	11	47 Geminor	p	4	2	n	0	18	1	Cluſtering L. ſc. ſt. many of equal ſize.

Notes to ſome Nebulæ and Cluſters of Stars.

I. 7. This remarkable appearance being no longer in the place it has been obſerved, we muſt look upon it as a very conſiderable teleſcopic comet. It was viſible in the finder and reſembled one of the bright nebulæ of the Connoiſſance des Temps ſo much, that I took it for one of them till I came to ſettle its place; but this not being done till a month or two after the obſervation, the opportunity of purſuing and inveſtigating its track was loſt.

I. 13.. The figures referred to, in the deſcription of this and ſome other nebulæ, may be found in the Philoſophical Tranſactions, vol. LXXIV. tab. XVII. p. 450.

I. 28. The numbers annexed to ſome of the nebulæ refer to the claſs and number of the preceding Catalogue: thus, II. 41. denotes that the 41ſt in the ſecond claſs is the third nebula, following the two here deſcribed.

I. 28. Near the 84. and 86. neb. of the Connoiſſance des Temps.

II. 6. This has probably been a teleſcopic comet, as I have not been able to find it again, notwithſtanding the aſſiſtance of a drawing which repreſents the teleſcopic ſtars in its neighbourhood.

II. 55. The preceding is the 85 of the Connoiſſance des Temps.

II. 84 6 or 8′ following the 100 of the Connoiſſance des Temps.

II. 118. Juſt following the 88. of the Connoiſſance des Temps.

II. 123. 124. The third is the 87th of the Connoiſſance des Temps.

III. 44. The following is the 60th of the Connoiſſance des Temps.

IV. 13. Before the value of the degree was more ſtrictly aſcertained, the two obſervations were thus:

21 Velpeculæ	f	2′ 6″	n	1° 51′
39 (*b*) Cygni	p	8 6	ſ	1 35

which, if there be no error in the place of the ſtars in FLAMSTEED's Catalogue, differ about 14′ in polar diſtance, for which reaſon in the ſecond Paper on the Conſtruction of the Heavens this nebula was put down twice, whereas it now appears, that both obſervations belong to the ſame.

V. 1. This nebula was diſcovered Sept. 23, 1783, by my ſiſter CAROLINE HERSCHEL, with an excellent ſmall Newtonian *Sweeper* of 27 inches focal length, and a power of 30. I have therefore marked it with the initial letters, C. H. of her name. See alſo V. 19. diſcovered Aug. 27, 1783, and VII. 13. diſcovered Feb. 26, 1783.

V. The *Front-view* is a method of uſing the reflecting teleſcope different from the Newtonian, Gregorian, and Caſſagrain forms. It conſiſts in looking with the eye glaſs, placed a little out of the axis, directly in at the front, without the interpoſition of a ſmall ſpeculum; and has the capital advantage of giving us almoſt double the light of the former conſtructions. In the year 1776 I tried it for the firſt time with a 10 feet reflector, and in 1784 again with a 20 feet one; but the ſucceſs not immediately anſwering my expectations, it was too haſtily laid aſide. By a more careful repetition of the ſame experiment I find now, that ſeveral other conſiderable advantages, added to the brilliant light before mentioned, make it ſo valuable a conſtruction that a judicious obſerver may avail himſelf of it at leaſt in all caſes where light is more particularly wanted; and from the experience of 30 ſweeps, which I have already made with it, I may venture to announce it to be a very convenient and pleaſant, as well as uſeful, way of obſerving. With regard to the poſition of objects, it differs from other conſtructions, by inverting the north and ſouth, but not the preceding and following.

Errata of the Catalogue.

The following nebulæ ſhould ſtand thus.

I. 54.	35 (ɾ) Andr.	f	18 36	ſ	1 26	1	pB. S. R. vgbM.
II. 1.	41 Aquarii	p	11 45	n	0 17	2	F. cL. mE. bM. er.
II. 239.	In the deſcription read						The 2d of two.

* * *

XXVIII. *Inveſtigation of the Cauſe of that Indiſtinctneſs of Viſion which has been aſcribed to the ſmallneſs of the Optic Pencil.* By William Herſchel, *LL.D. F.R.S.*

Read June 22, 1786.

SOON after my firſt eſſays of uſing high powers with the Newtonian teleſcope, I began to doubt whether an opinion which has been entertained by ſeveral eminent authors, "that "viſion will grow indiſtinct, when the optic pencils are leſs "than the 40th or 50th part of an inch," would hold good in all caſes. To judge according to ſo rigid a criterion, I perceived that I was not intitled to ſee diſtinctly with a power much more than about 320, in a 7-feet teleſcope which bore an aperture of 6,4 inches; whereas in many experiments on double ſtars I found myſelf very well pleaſed with magnifiers that far exceeded ſuch narrow limits. This induced me, as it were, by way of apology to myſelf, for ſeeing well where I ought to have ſeen leſs diſtinctly, to make a few experiments on the ſubject of the diameter of optic pencils. It occurred to me, that an opinion which limits them to any given ſize cannot be ſupported by theory, which does not determine on ſubjects of this nature, but muſt be decided, like many other phyſical queſtions relating to matters of fact, by careful experiments made upon the ſubject. The way, therefore, to come at truth, in a caſe which ſeemed to me of conſiderable importance, lay ſtill open to me, as it had done to former obſervers;

ſervers; and I thought myſelf authoriſed, according to a Carteſian maxim (*Dubia etiam pro falſis habenda*), to ſuppoſe, for a while, the ſize of optic pencils, requiſite for diſtinct viſion, intirely undecided.

The firſt opportunity I had of making the propoſed experiments was in the year 1778, and the reſult of them proved ſo deciſive that I have never ſince reſumed the ſubject; and had it not been for a late converſation with ſome of my highly eſteemed and learned friends, I might probably have left the papers, on which theſe experiments were recorded, among the reſt of thoſe that are laid aſide when they have afforded me the information I want. But a doubt ſeeming ſtill to be entertained on the ſubject of the ſmallneſs of the optic pencils, it may now be proper for me to communicate theſe experiments, that it may appear how far the concluſions I have drawn from them are warranted by the facts on which I ſuppoſe them to reſt.

Experiments with the naked eye.

Exp. 1. Through a very thin plate of braſs I made a minute hole with the fine point of a needle; its magnified diameter, very accurately meaſured under a double microſcope, I found to be ,465 of an inch, while under the ſame apparatus a line of ,05 in length gave a magnified image of 3,545 inches. Hence I concluded, that the real diameter of the perforation was about the 152d part of an inch. Through this ſmall opening, held cloſe to the eye, I could very diſtinctly read any printed letters on which I made the trial. Proper allowance muſt be made for the very inconvenient ſituation of the eye, which by the unuſual cloſeneſs to the paper cannot be expected to ſee with its common facility. Beſides, the continual motion of the letters, which is required on account of the ſmallneſs of the field of view, muſt needs take up a conſiderable time.

Exp. 2. In ſome other pieces of braſs I made ſmaller holes; and among many, that were meaſured with the ſame accuracy as in the former experiment, I found one whoſe magnified diameter was ,29: hence the real diameter could not exceed the 244th part of an inch. Through this opening I could alſo read the ſame letters; but the difficulty of managing ſo as not to intercept all the incident light, as well as the very uneaſy ſituation of the eye, were ſufficient reaſons for not carrying the intended experiments any further under this form. Beſides, I ſhould hardly have allowed them to be fair, if, on a further contraction of the hole in the braſs plate, an indiſtinctneſs had come on; as we might well have ſuſpected at leaſt two other cauſes, beſides the ſmallneſs of the pencils, to contribute to ſuch an imperfection; *viz.* want of light, and a deflection of it on the contracted edges of the hole.

Microſcopic Experiments.

Exp. 3. I had now recourſe to a double microſcope, conſiſting, for ſimplicity's ſake, of only two lenſes. The focal length of the eye-glaſs, carefully aſcertained by an object half a mile off, being ,9; the diſtance of the object-glaſs from the eye-glaſs 9,36; and the aperture of the object-glaſs ,0405. Hence we compute that the diameter of the optic pencil, when it entered the eye, could not exceed the 232d part of an inch; yet with this conſtruction I ſaw very diſtinctly every object I placed under the microſcope.

Exp. 4. I reduced the aperture of the object-glaſs to ,013; hence the pencil was found to be the 724th part of an inch; and yet I ſaw with this conſtruction very diſtinctly every object that was placed under the magnifier.

Exp. 5. I made a ſecond reduction of the aperture of the object-glaſs, ſo that now it was no more than ,0052; and therefore the optic pencil leſs than the 1800th part of an inch; and yet I could very well count the briſtles on the edge of the wing of a fly, and diſtinguiſh their length and thickneſs.

Exp. 6. Changing the conſtruction of the microſcope, I now reduced the pencils by an increaſe of power. Solar focus of the eye-glaſs ,52; diſtance between the object-glaſs and eye-glaſs 7,6; aperture the ſame as in the third experiment. This gave me a pencil of the 336th part of an inch, with which I ſaw very diſtinctly.

Exp. 7. Applying now the reduced aperture of the fourth experiment, I had a pencil of the 1139th part of an inch, with which I ſaw very well.

Exp. 8. I changed the eye lens for another of ,171 focal length; the object glaſs and diſtance between the two lenſes remaining as in the two laſt experiments; aperture ,02. This gave a pencil of the 2173d part of an inch, with which I could count, or rather ſucceſſively ſee, the briſtles before-mentioned very well; the field, on account of the great power, not taking in more than two large and a ſmall one at a time.

Exp. 9. I was now convinced, that we may ſee diſtinctly with pencils incomparably leſs than the 40th or 50th part of an inch; and indeed ſo far from expecting any obſtruction to diſtinct viſion from the ſmallneſs of the pencils, it appeared to me now as if their ſize might in future be intirely left out of the account. With a view, however, of ſeeing what other cauſe might bring on that indiſtinctneſs which had been aſcribed to the ſmallneſs of the optic pencils, I continued theſe experiments with a variation in the apparatus, and uſed now an object lens of a different focal length; the aperture and other particulars being as in the 4th experiment. By this conſtruction, which gave me a pencil of the 724th part of an inch, I could ſee objects very well; but though they appeared diſtinctly, they were not ſo ſharp on the edges as one would wiſh to ſee them. This being compared with the 4th experiment, it appeared that, with equal pencils, unequal degrees of diſtinctneſs may take place; and a pretty ſtriking circumſtance, which ſerved to lead me in the following experiments, was, that the ſmalleſt power gave me the leaſt diſtinct image; notwithſtanding, from former trials, the goodneſs of the lenſes I employed could not be doubted.

Exp. 10. On an examination of circumſtances it occurred to me, as indeed I had already before ſurmiſed, that a certain proportion of aperture might be neceſſary to a given focal length of an object-lens or ſpeculum; and that a failure in this point might probably bring on that indiſtinctneſs which had been aſcribed to the ſmallneſs of the pencils. In order, therefore, to put this to a trial, I uſed now an object-lens of 1,25 focal length, with an aperture confined to ,01; the reſt of the apparatus being as in the 3d, 4th, and 5th experiments. The pencil in this caſe was about the 1000dth part of an inch; and though by a different conſtruction I had already ſeen very well with a pencil of not half that diameter, I found this to give me, as now I had reaſon to expect, a very indiſtinct picture, ſo much ſo indeed, that it could hardly be called a repreſentation of the object.

Exp. 11. Increaſing the aperture of the object-lens to ,0124, I had a pencil of the 758th part of an inch, but could ſee no better with it.

Exp. 12. Proceeding in the track now pointed out to me, I admitted an aperture of ,017, which gave a pencil of the 550th part of an inch, but could ſee not much better with it than before.

Exp. 13. On a farther increaſe of the aperture to ,0231, and a pencil of the 406th part of an inch, I ſaw a little better; but ſtill had not diſtinctneſs enough even to ſee the briſtles before-mentioned at all. Hence we may conclude, that, in ſuch conſtructions as the preſent one, the aperture of the object-glaſs muſt bear a conſiderable proportion to its focal length; ſince the 54th part (for ,0231 : 1,25 :: 1 : 54) is here not nearly ſufficient.

Exp. 14. To the ſame apparatus I applied a higher power, by an exchange of the eye-glaſs; but the indiſtinctneſs remained as before.

Exp. 15. Returning again to the former conſtruction, I admitted an aperture of about ,037; and having now a pencil of nearly the 250th part of an inch, I could but juſt perceive ſome of the large briſtles, which ſhews that even the 34th part (for ,037 : 1,25 :: 1 : 34) of the focal length is not a ſufficient aperture for object-lenſes that act under ſuch circumſtances as the preſent.

So far I have only related experiments that were made in the year 1778; and my opinion that the ſmallneſs of the optic pencils could be no objection to ſeeing well being thus ſupported by evident facts, I heſitated not, in a Paper on the Parallax of the Fixed Stars (Phil. Tranſ. vol. LXXII. p. 96.) to affirm, that we might ſee diſtinctly with pencils much ſmaller than the 40th or 50th part of an inch. It did not appear to be neceſſary, nor would the ſubject of that Paper permit me to enter into a detail of experiments; but having, in the courſe of my reading about that time, met with an account of ſome very ſmall globules made for microſcopic uſes, I contented myſelf with an inſtance of ſmall pencils taken from them. I ſhall, however, now proceed juſt to hint at a few inferences that may be drawn from theſe related experiments; as, upon a mature conſideration, we may find reaſon to believe they point out a cauſe of indiſtinctneſs of viſion hitherto never noticed by optical writers; and which, when properly inveſtigated, cannot but influence, and in ſome reſpects contribute to the improvement of, our theories in optics. For, admitting that every object-glaſs or ſpeculum, whoſe aperture bears leſs than a certain ratio to its focal length, will begin to give an indiſtinct picture, it will follow, that while former opticians have been endeavouring to diminiſh the aberrations ariſing from the ſpherical figure, and the different refrangibility of rays, by increaſing the focal length, they have been unaware of expoſing themſelves to the conſequences of the cauſe of indiſtinctneſs here pointed out. And till its influence ſhall be well aſcertained and brought to a proper theory, we muſt ſuſpect that ſuch tables as thoſe which are given in our beſt authors of optics, pointing out an aperture of leſs than 6 inches for a glaſs of 120 feet focal length (or a ratio of 1 to 240) muſt be far from having that degree of perfection which may yet be obtained. No wonder that teleſcopes, made according to theories or tables, where one of the cauſes of indiſtinctneſs is unſuſpected, and therefore left out of the account, can bear no ſmaller pencil than the 40th or 50th part of an inch! If then, on one hand, by increaſing our apertures we certainly run into great imperfections, we ought nevertheleſs alſo to conſider what dangers, on the other, we may incur by leſſening them too much.

As ſoon as convenient, I intend experimentally to purſue this ſubject, in order to obtain proper *data* for ſubmitting this cauſe of optical imperfection to theory; at preſent my engagement with the work of a 40-feet reflector will hardly permit ſo much leiſure; and till I ſhall have repeated, extended, and varied theſe experimental inveſtigations, I would wiſh them to be looked upon as mere hints that may afford matter for future diſquiſitions to the theoretical optician.

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